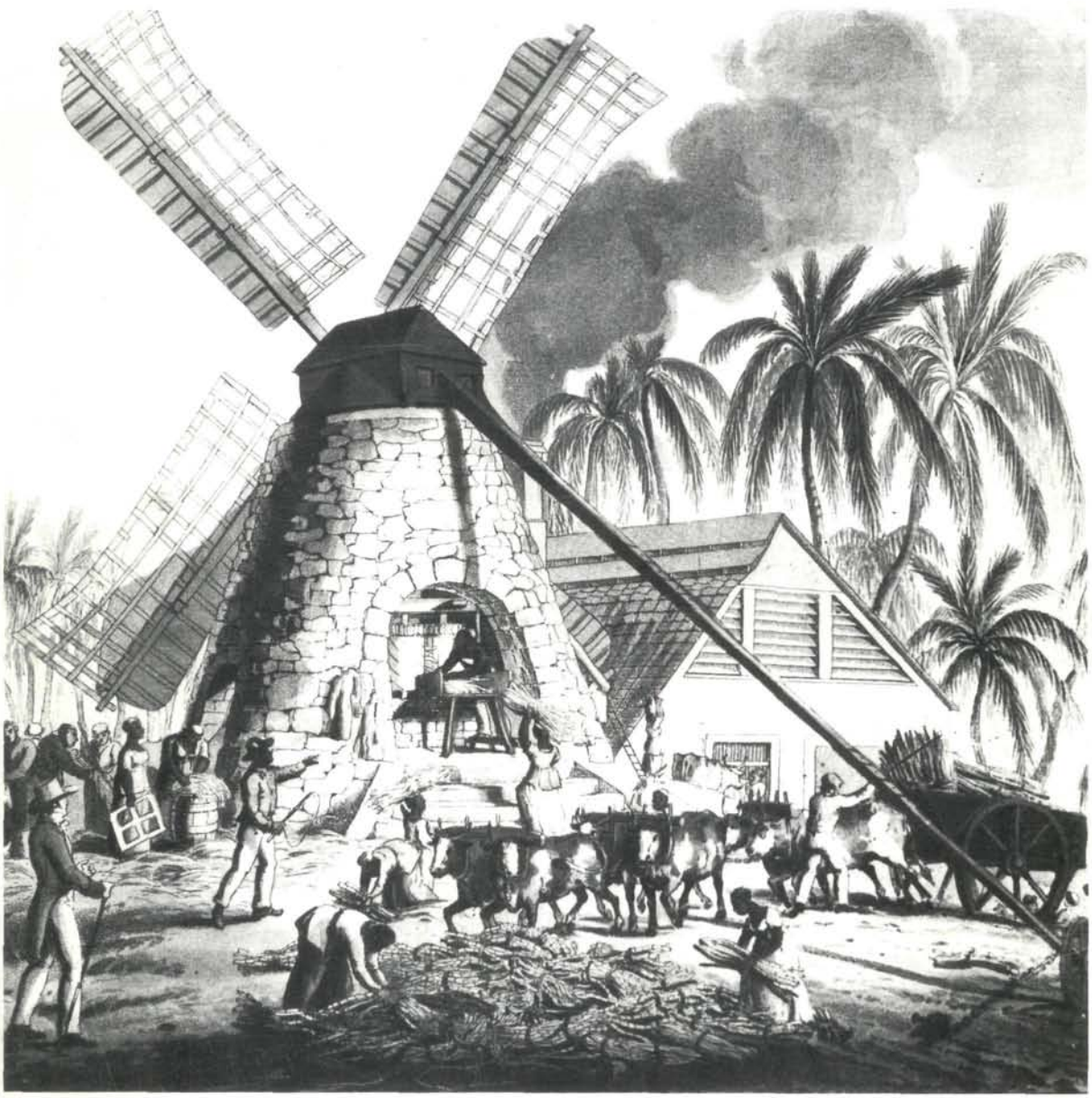
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The Fourth and the Fourteenth Centuries

JOSEPH R. STRAYER

EVERY CIVILIZATION of which we know anything has met at least one, and usually more than one, major crisis in its history. After centuries of relatively calm and continuous development, cracks appear in the value system and the social structure; cracks that are too wide to be spanned by the bridge of tradition and too deep to be filled by the rubble of rejected utopias and patchwork reforms. In such a crisis there are only two possible outcomes—drastic reorganization or even more drastic disorganization. Our own civilization seems to be in the midst of such a crisis. A historian may find some comfort, and perhaps even some enlightenment in considering earlier crises in the history of civilizations.[1]

In this essay I want to discuss two periods of crisis—the fourth and the fourteenth centuries in Western Europe. I need hardly say that my centuries are very flexible chronological units; much that happened before 300 A.D. contributed to the crisis of the fourth century, and much that happened after 1400 was a continuation of the crisis of the fourteenth century. Nor do I mean to imply that only the history of Western Europe is relevant for our purposes. We could certainly learn as much from the crises that have periodically shaken Chinese civilization. But I know very little about Chinese history and I do know something about European history. Moreover, the two periods that I have selected seem to me to display most of the problems that occur in crises in any civilization. One final explanation: when I say Western Europe, I mean very nearly what is meant by the phrase as it is used today. In discussing the fourth-century crisis I am excluding the eastern part of the Roman Empire, which certainly shared in the crisis but solved it in a very different way, and North

This is an expanded version of a presidential address delivered by Mr. Strayer at the annual meeting of the American Historical Association, New York, December 28, 1971.

[1] Perhaps one sign that we are indeed facing a major crisis is the fact that during the last half century so many men of great intellectual power have been considering the problem of the decline or transformation of civilizations. The work of Spengler, Sorokin, Toynbee, Kroeber, and other generalists is too well known to require specification, though I should call attention to the incomplete but stimulating ideas of Rushton Coulborn. See, for example, "Structure and Process in the Rise and Fall of Civilized Societies," *Comparative Studies in Society and History*, 8 (1966): 404–51.

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Africa, which was part of the West when the crisis started but was pulled over to the East by Byzantine and Muslim conquests before it ended. In discussing the fourteenth century I am excluding East Central Europe, where both the nature of the crisis and its results differed from the Western pattern—most so in Poland-Lithuania, least (as usual) in Bohemia. If an analysis of a crisis is to have any validity it must deal with areas that remained relatively homogeneous over long periods.

We are thus seeking in the crises of the fourth and fourteenth centuries in Western Europe examples of processes that have occurred at other times in other civilizations. We can begin by looking for similarities between the two crises in Europe; they are not difficult to find. In both periods Western Europe was suffering from a prolonged economic depression. In both periods there was a decline in population that added complications to the problem of economic readjustment. Moreover, what began as a gradual decline in population was intensified by epidemics of plague, and the psychological shock of epidemics may have been worse than the economic consequences. It is true that the plague in the Roman world came before the crisis, during the second and third centuries,[2] while the plague in the medieval world came after the crisis was well under way. On the other hand, the economic and psychological problems caused by the fourteenth-century plague lasted for several generations, so that it is not unreasonable to suppose that the effects of the third-century plague had something to do with the crisis of the fourth century.

Some parallels may also be noted in the political field. In both centuries civil wars weakened central governments, allowed local magnates to gain a considerable degree of autonomy, and left many regions a prey to wandering bands of mercenary or foreign soldiers. In such regions living conditions deteriorated, and neither local, provincial, nor central authorities were able to provide any acceptable degree of security. In both centuries barbarians were pressing hard against the eastern gates to Europe, and in each century a disastrous defeat broke the Danube line and opened a path to Western Europe (Adrianople in 378 and Nicopolis in 1396). It is true that the Turkish threat, in the long run, proved to be less dangerous than the earlier Gothic threat, but this fact was not immediately apparent to Europeans of the late fourteenth and the fifteenth centuries.[3] And whatever the degree of the threat, political breakdown had gone so far in each period that no concerted effort could be made by Western Europeans to keep the danger far from their borders.

More perilous to the civilization of Western Europe than these economic and political problems was the decline in morale. One thinks im-

[2] J. C. Russell, "Late Ancient and Medieval Population," *Transactions of the American Philosophical Society*, n.s. 48, pt. 3 (1958): 37.

[3] Nicolae Iorga, *Philippe de Mézières et la croisade au XIVe siècle* (Paris, 1896); A. S. Atiya, *The Crusade in the Later Middle Ages* (London, 1938), chs. 7–10.

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mediately of religion in this context, and it is obvious that in both centuries many men were seeking a new and more satisfying form of religious experience, and few were finding it. But religion is more than a set of individual experiences, important as they are. It is also a set of shared beliefs that make it possible for large numbers of people to work together, to endure the restraints and meet the demands imposed by organized society, to be willing to play a part in an intricate process that no one can fully understand, to find satisfaction in life without being flooded by material benefits. Most historians have realized the importance of religion; few have extended its definition widely enough.[4] We all know that there can be a religion of the state, but there can also be a religion of science, a religion of technology, and even, for some peoples and some periods, a religion of good manners. Religion in this sense is the mortar that holds a society together and the vision that leads men to believe that their society is worth preserving and handing on to their children.

In both the fourth and the fourteenth centuries the mortar was crumbling and the vision was becoming dim. There was little confidence in secular organizations and not much more in religious bodies. One may wonder whether a Roman society really existed in the West by the time of Theodosius. If there was a Roman society it was not a society that anyone would die to preserve. That task was left to barbarian mercenaries, allies, and hereditary frontier guards, men who were not a part of the society that they were supposed to defend. What is even more indicative of a decline in morale, Roman society in the West was not a society that anyone would work to preserve. Even the last Western emperors made no great effort to preserve the unity of the West, and the interests of the senatorial class had become largely provincial several generations before the emperors abandoned Britain and Gaul. At the local level the key institution of the *civitas* was withering away. Men had to be forced to perform their civic duties, which meant that the work was done without conviction, and when force was not available the work was not done at all. The tendencies toward the fragmentation of large political units and the simplification of political institutions that ran all through the early Middle Ages were already apparent in Western Europe at the end of the fourth century.

One finds the same tendency toward fragmentation in the areas of social and economic behavior. Rostovtzeff may have exaggerated the antipathy between the peasants and the town-dwellers,[5] but certainly

[4] Toynbee's estimate of the importance of the "higher religions" is well known, but he thinks mainly in terms of "religious" religions, if I may use the phrase. A definition that is closer to the one in the text, though still not entirely satisfactory, may be found in Rushton Coulborn, "The Concept of the 'Conglomerate Myth,'" *Proceedings of the Tenth International Congress of Philosophy* (Amsterdam, 1949), 1: 74–81.

[5] M. I. Rostovtzeff, *Social and Economic History of the Roman Empire* (London, 1926), ix, 442–48.

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neither group was much concerned with the troubles of the other. The senatorial class sought to preserve the way of life of the cultivated country gentleman, a tradition that cut it off sharply from other classes. And while the extent of local self-sufficiency was surely not so great in the fourth century as it was to be later, there was not very much economic interdependence in the provinces of the West. Southern Gaul could flourish while the northeast was devastated, and Italy was little affected by transalpine wars and rebellions.

Some of the same problems can be found in the fourteenth century. The danger of social and political fragmentation reappeared; uprisings and civil wars threatened the unity of towns, principalities, and kingdoms. There were some bitter class conflicts, notably the *Jacquerie* in France, but equally dangerous and even more numerous were conflicts caused by splits within each class—new rich against old rich, proroyalist nobles against antiroyalist nobles, Urbanist clergy against Clementist clergy. Personal ambitions, parochial jealousies, and factional rivalry obscured the ideal of the general welfare. All loyalties were shaken, even loyalty to the universal Church. The religious unity of Western Christendom was a thin veil covering deep divisions, and the divisions became even more apparent with the coming of the Great Schism in 1378. In 1400, as in 400, a good many men must have wondered whether their society could or should endure.

So much for the parallels. Some of them, as you have doubtless realized, are a little forced. They are forced, first of all, because Western Europe in the fourteenth century was a completely autonomous, self-sufficient civilization, and Western Europe in the fourth century was only an outlying part of a much larger whole—the Mediterranean civilization. A considerable part of the troubles of the fourth century was caused by the fact that Western Europe, quite unintentionally, was withdrawing from the Mediterranean civilization. The East met the crisis of the fourth century by transforming its institutions and beliefs. The West was unable to keep up with these transformations (for example, rapid conversion to Christianity), and its own resources were too meager to produce satisfactory substitutes. In the fourteenth century there was no withdrawal problem, and the West had greatly enriched its intellectual and spiritual resources.

In the second place, the background of the two crises was different. The Mediterranean civilization had already experienced a dangerous crisis in the third century and had survived only through the establishment of a military dictatorship. Western European towns, the vital centers of the Mediterranean civilization, had been weakened by this crisis. The unity of the West had been threatened by the establishment of a practically independent empire in Gaul, an empire that lasted almost twenty years. No Westerner could view the immediate past with any satisfac-

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tion. And if he tried to forget the red gash of the fifty years of anarchy (235–85 A.D.) and sought to revive the traditions of the early Empire he was guilty of sentimental antiquarianism. Yet the senatorial class, the wealthiest and most influential group in the West, committed precisely this fault, which is one reason why this class was so helpless when the fourth-century crisis intensified.

In contrast, to the men of the fourteenth century the immediate past appeared to be a golden age. There had been no serious wars between 1215 and the 1290s; there had been a steady increase in production with just enough inflation to keep the economy moving; the ideals of the society had found almost perfect expression in art and philosophy and had been realized in the lives of saints and kings. It was not sentimental antiquarianism in the fourteenth century to appeal to the principles of Magna Carta, or to the even-handed justice of St. Louis, or to the civic virtue of the early Florentines. Such appeals had an effect because there had been no breach of continuity between the thirteenth and the fourteenth centuries. And with a golden past only two or three generations behind them it was possible for the people of Western Europe to hope that a rosy future would not be much further away.

They were not entirely wrong in their hope, and this fact indicates another important difference between the two crises. As Rushton Coulborn once wrote: "There are long transitions and short ones, and the long ones are always deep at the same time and the short ones shallow."[6] The fourth-century crisis was more severe than the fourteenth-century crisis because it lasted longer, and it lasted longer because it was more severe. Many scholars have been fascinated by the history of Western Europe in the early Middle Ages. They have unearthed every scrap of evidence that would prove that there were capable rulers, learned scholars, first-rate artists, and competent businessmen in the fifth, sixth, and seventh centuries. The evidence of individual accomplishments is quite acceptable, but it does not prove that there was a Western European civilization or an organized society during those centuries. An old civilization was crumbling and a new one being created, but it took many centuries before the transition was accomplished. For example, Christianity was an essential ingredient of the new civilization, and Christianity spread very slowly in the West. Western Europe was not Christian, even in a nominal sense, until the Carolingian period, and Christianity began to have a real impact on the European system of values only in the eleventh century. In short, the work of reorganizing European society after the collapse of the Empire in the West took four or five hundred years. And because the rebuilding took so long, the civilization that finally emerged was very different from its predecessor.

[6] Rushton Coulborn, "The Ancient River Valley Civilizations," in Shirley H. Engel, ed., *New Perspectives in World History* (Washington, 1964), 127.

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In contrast, the civilization of Western Europe was shaken but not shattered by the crisis of the fourteenth century, and while some major repairs were needed there was no complete rebuilding. The Western Europe that emerged from the crisis was quite clearly a direct descendant of the Western Europe that had entered the crisis. Some old institutions and patterns of life were modified; some new institutions and habits of thought were introduced; but in most areas changes were neither sweeping nor catastrophic. Indeed, continuity was so great and change so small that some ardent medievalists have denied that there was any significant alteration in European society. This point of view is understandable but, I think, mistaken. The men of the sixteenth century believed that they were quite different from the men of the thirteenth century. Certainly they understood the thirteenth century less well than they thought they did, and the fact that they could misunderstand so many aspects of thirteenth-century life shows that some significant changes had taken place.

Following Coulborn's formula, a shallow transition should be a brief one, and this was certainly true of the fourteenth-century crisis. Some signs of spiritual malaise and of economic depression were evident in the last quarter of the thirteenth century. The spiritual malaise was reinforced by the controversies that swirled around Boniface VIII and John XXII; the economic problem was intensified by the great European famine of 1315–17. But the real drop in morale came only in the second quarter of the fourteenth century, with civil wars, the Hundred Years' War, the failure of the leading Italian banking houses, the growing discontent with the papal residence in Avignon, and above all with the Black Death. The aberrant social behavior caused by the drop in morale continued into the fifteenth century—how far into that century is a matter for scholarly debate. The rate of recovery varied from country to country and from one type of activity to another. The essential signs of recovery were a growing sense of confidence and an increasing ability to cooperate, nebulous factors that are almost impossible to measure. But whatever the causes and the pace of recovery, the worst was over by the last quarter of the fifteenth century, and by that time the new patterns of European life and thought could be clearly discerned. Thus, if we spin the crisis out to its fullest extent, it lasted about two centuries. If we limit it to the period of extreme distress, it lasted less than one hundred and fifty years. This is not a short time in terms of human anguish—a man born in 1300 had every reason to be concerned about his own fate and that of his children's children. But it was a short time in terms of the history of civilizations and a short time in comparison to the centuries of groping, false starts, precarious successes, repeated setbacks, and continuing strain that followed the decline of the Roman Empire in the West.

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So far I have been telling a familiar story. Most historians would probably accept the main outlines, though they would quarrel about some of the details. But now I should like to consider why the two crises had such different outcomes, and here I venture on hazardous ground. The only explanation that makes any sense to me is based on an appraisal of human beliefs and motives. Historians disagree strongly about the beliefs and motives that have affected the course of events in our own century, and yet we have more evidence about the nature of contemporary beliefs and motives than we have for any earlier period. How can we speak with any confidence about the beliefs and motives of generations that vanished long ago? The evidence is scanty and heavily class-biased; much of the time we must argue from effect to cause. Even when we are sure about the existence of a belief, it is hard to estimate its intensity.

If we seek other types of explanation, however, we should realize that the evidence is no better. We have no figures for medieval agricultural or industrial production that a conscientious economic historian could accept, and yet we speak of economic growth and decline. Our estimates of medieval population are mere approximations—if we could prove that the average margin of error was no greater than fifty per cent we would all be happy[7]—and yet we have some reason to say that at certain times certain areas were overpopulated and others underpopulated. Much can be done with fragmentary evidence, even though it requires us to take long and dangerous inductive leaps.

Fortunately I am trying to explain not the causes of the crises but the differences in rates of recovery, and for this problem the most exact economic or demographic statistics would not give the answer. As we have every reason to know, increasing GNP per capita does not guarantee the stability of a society. Underpopulation may be associated with stagnation or with rapid growth, overpopulation with breakdown or with expansion. However good the statistical evidence, we shall always have to look behind it for the intangibles of beliefs and attitudes, and no evidence about beliefs and attitudes will ever be completely satisfactory.

Embarking, then, on this dangerous inquiry, the first thing that strikes one about Western Europe in the fourth and early fifth centuries is the weakness of religious motivations. This may seem a strange thing to say about the period that saw the conversion of Constantine, the Council of Nicæa, and the Arian controversy. But it should be remembered that these developments touched very few people in Western Europe. In this region the majority of the population was only nominally Christian, if it

[7] We have better information for England than for any other medieval country, yet J. C. Russell estimates a peak population in 1348 of about 3,700,000 (perhaps a little higher ca. 1300). *British Medieval Population* (Albuquerque, 1948), 246. M. M. Postan, on the other hand, thinks that the peak population ca. 1300 was nearer seven million. "Medieval Agrarian Society in its Prime: England," in M. M. Postan and E. E. Rich, eds., *Cambridge Economic History of Europe*, I (2d ed.; Cambridge, 1966): 562.

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was Christian at all. Leadership of the Church was in the hands of an aristocracy that was remote from the common people, increasingly aloof from politics, and suspicious of enthusiasm.[8] Strong convictions about religion and close connections between religion and the organization of society were largely confined to the East. The Christian Roman Empire was a reality in the East, but only a façade in the West. The emperor in Constantinople gained power and prestige by being the protector of the faith and the patron of the Church. The emperor in Ravenna gained almost nothing by being a Christian.

It is true that in both East and West Christianity at its most intense level was more concerned with saving the individual than with saving society. In almost everybody's eyes the best Christians were the dropouts —the men who saw no good in their society, wandered off to join communes, and lived on handouts. But even here there was a contrast between East and West. The Western monks remained outside their society; they took almost no part in social or political activity. The Eastern monks, in spite of their professed contempt for the world, were still passionately interested in the affairs of their society. Again and again they descended on the cities to force their ideas on religious and secular authorities. In the end they took root in the cities, where they remained a powerful force. In short, Christian ideals and Christian leaders played a major role in transforming the old Greco-Roman civilization of the East into Byzantine civilization. In the West Christian leaders did little to stop the collapse of their civilization and for a long time did little to transform it into a new civilization.

If Christianity had little influence in Western Europe, the various forms of secular religion had less. Respect for the emperor declined steadily; loyalty to the Empire almost vanished; veneration for Rome was merely part of the antiquarian cult of the aristocracy. A vast apathy engulfed the larger part of the population; nothing was going to work and nothing was worth working for. The minority of concerned men could only fight to preserve the status quo, a battle that can never be won. They moved in ever-narrowing circles, both geographically and culturally, until at the end they could hope for no more than to preserve a little Latin learning and a few Roman institutions in a single *civitas*. Even these modest goals proved impossible to attain. The people of the West had lost almost all interest in their old civilization, and they let it crumble without making any effort to save it or to transform it.

In the fourteenth century, on the other hand, the problems were not apathy but exaggerated sensitivity, not lack of interest in social organiza-

[8] Samuel Dill, *Roman Society in the Last Century of the Western Empire* (London, 1898), 5–22, 179–85; O. M. Dalton, *The Letters of Sidonius* (Oxford, 1915). As late as the sixth century Gregory, bishop of Tours, prided himself on his senatorial and episcopal ancestry; his predecessor and all but five of the previous bishops of Tours had been connected with his family.

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tion but strong differences of opinion about how to make social organizations work, not the absence of conviction but frustration caused by the poor quality of leadership. Few people were content with the existing situation, and many must have wondered if there could ever be any real improvement. But while outbursts of despair were common enough, despair was not the dominant mood of the fourteenth century. Instead men talked endlessly, tirelessly, and apparently vainly about "reform." Even the dropouts of the fourteenth century, unlike those of the fourth, did not abandon their society. They tended to congregate in urban centers rather than flee to the wilderness; many engaged in reform movements; many performed useful social functions such as teaching. There was a danger of fragmentation of political units during the fourteenth century, but this danger was not caused by lack of interest in preserving any organized community. Instead it resulted from the conflicting demands of many communities. Which community should a man serve—the little community of the parish or the guild, the larger community of the town or the province, or the great community of the kingdom or the principality? It was not easy to reconcile these obligations, yet enough men managed to do so to prevent either anarchy or breakdown. This is even more striking when we remember that all fourteenth-century communities depended on the unpaid assistance of their members, since there were never enough paid functionaries to do the work. If there had been complete disaffection from fourteenth-century society it would have been easy enough to destroy it: ordinary men would simply have failed to perform their civic duties. This is very much what had happened at the end of the fourth century. It did not happen at the end of the fourteenth century because the inhabitants of Western Europe were willing to work to keep their societies functioning.

They were willing to work because they still had hope, and they had hope because they still believed in their religions. Almost everyone, including the most violent and turbulent members of society, was a Christian. It is true that almost every Christian criticized the Church bitterly at one time or another, but not because he lacked faith. It was, rather, that too much was expected of the faith and of the Church that was the visible embodiment of the faith. There was incessant argument about what the Church should do and could do, but there was agreement on two points: the Church was a necessary part of society, and the health of society depended on the health of the Church.

Much the same attitudes existed toward the state. By the end of the thirteenth century almost everyone believed that the state was necessary for human welfare, and yet a very large number of people believed that their own state was not doing a very good job. This dissatisfaction was not due to conflicting ideologies but to exaggerated ideas of what the weak and understaffed states of the fourteenth century could do. Governments

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were not very farsighted, not very honest, not very efficient. Everyone longed for the "good governance" that would cure the ills of the age, though no one was very sure how it was to be obtained. Meanwhile subjects paid taxes and obeyed orders. When leadership was uncertain or perverse, as it often was, willingness to pay and to obey declined, but never to the point at which the state utterly collapsed. The reservoir of loyalty never became entirely empty; the hope for good governance was never quite abandoned. And it was this basic loyalty, this willingness to give at least minimum obedience to weak or corrupt governments, that shows how strong the secular religion of the state had become.

The frequent rebellions and revolutions of the fourteenth century do not invalidate but, rather, confirm this judgment. They were a sign of vitality, not of despair. People without hope do not rebel; there were no popular uprisings during the dying years of the Western Roman Empire. Many of the fourteenth-century rebellions were simply factional struggles for power led by groups of nobles or of well-to-do businessmen who felt that they deserved more of the spoils and perquisites of office. Nevertheless, the victorious factions usually found it advisable to promise reform, even if this meant no more than dismissing a few unpopular functionaries and making a few changes in the tax structure. Some urban uprisings and peasant rebellions had more radical goals: they wanted to decrease the income of the rich and increase the income of the poor. But in the absence of any competing ideology they could be radical only within the existing political framework. When the workers of Florence rebelled in 1378, they simply took over the Florentine state, and they made so few changes in its basic structure that they quickly lost control. When the English peasants and artisans rebelled in 1381, they could think of no better way to obtain their objectives than to use the existing monarchy. They accepted King Richard II as their leader and showed touching confidence in the charters that he issued. The only chance for the *Jacquerie* in the 1350s was to work with the members of the Paris bourgeoisie who were trying to gain control of the royal government. When this alliance collapsed the *Jacquerie* became a purely destructive force and was quickly crushed. In short, in spite of all their grievances, politically conscious people wanted to preserve their states. Uprisings were warnings to the state that it must do its job better; they were not attempts to create a completely new society.

Belief in the religion of the Church and the religion of the state was almost universal. Belief in the religion of science—science in the broad and original meaning of the word—was less widespread, but it was a powerful force. The University of Paris, accepted by everyone as the greatest center of learning in the West, had an influence equal to that of many kings and princes. Its pronouncements, especially during the period of the Great Schism, were always treated with respect and were

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often accepted as guides to policy. Laymen thought it was their duty to encourage learning. Charles V of France accumulated a remarkable library; statesmen and administrators founded hundreds of schools and colleges.[9] The people who were attracted by the religion of science believed that it would provide new, interesting, and useful knowledge about everything from the Grace of God to the proper construction of a Latin sentence. Actual achievements of fourteenth-century scholars were slight —the Merton formula for accelerated motion was perhaps their greatest accomplishment[10]—but the significant fact was that they believed that achievement was possible. In the fourth and fifth centuries the prevailing attitude was that there was little new to be discovered and that the chief task of scholars was to preserve existing knowledge. In the fourteenth century a number of men, from theologians to physical scientists, believed that knowledge could be expanded.

One can speak of a religion of science in the fourteenth century; perhaps one can also speak of the beginning of a religion of technology. Many men (how many we shall never know) believed that they could improve their processes of production, but they were not writers themselves, and they were largely ignored by those who did write. We can only guess at their motives and their aspirations. We can say that the rate of technological change increased in the fourteenth century and that the changes, unlike those of the early Middle Ages, affected industry more than agriculture. Improvements in mining, in blast furnaces, in metalworking, in glassmaking, and in textile production made possible the creation of new industries that took up some of the slack caused by the Great Depression of the fourteenth century. Here again the difference in spirit between the fourth and the fourteenth centuries is striking. There was not much interest in technology in the fourth century, although the nomads pushing in from Central Asia brought with them an entirely new set of techniques for using animal power. There was obviously increasing interest in technology in the fourteenth century, an interest that finally surfaced in the fifteenth century with drawings of machines and little treatises on engineering.[11] And I might add that while there were some protests, especially by conservatives in the textile industries, on the whole the men of the fourteenth century were not afraid of technological inno-

[9] It is significant that not only famous and wealthy statesmen, such as William of Wykeham, founded colleges, but also minor officials and professional men, such as the lawyer Raoul de Presles in 1314 (see F. J. Pegues, *The Lawyers of the Last Capetians* [Princeton, 1962], 159–60), or Andreas And, provost of the chapter of Upsala from 1291 to 1313 (see Astrik L. Gabriel, *Skara House at the Medieval University of Paris* [Notre Dame, 1960], 23–29). In the thirteenth century such men would have given their money to monasteries or founded small collegiate churches.

[10] Marshall Clagett, *The Science of Mechanics in the Middle Ages* (Madison, 1959), 255–68.

[11] See, for example, Lynn White, jr., "Kyeser's 'Bellifortis': The First Technological Treatise of the Fifteenth Century," *Technology and Culture,* 10 (1969): 436–41. This treatise was completed in 1405. For later works of the fifteenth century see Ladislao Reti, "Francesco di Giorgio Martini's Treatise on Engineering and Its Plagiarists," *ibid.,* 4 (1963): 287–98.

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vations. In this area at least they saw hope in change—an attitude that has not always existed in other societies.

The most remarkable aspect of the survival of faith in the Church, loyalty to the state, and devotion to learning is that these three beliefs[12] remained strong in spite of the mediocre—or worse—record of scholars, prelates, and rulers. As has already been suggested, scholars did their best work in the highly abstract fields of philosophy (including natural science) and theology. In practical matters they were less successful. Treatises on political theory did little to improve actual governments,[13] and the consultations of the University of Paris did not heal the Schism. The clergy talked earnestly about reform and about restoring the unity of the Church, but there was little reform and no unity at the end of the century. Secular rulers did not do much better. They made some improvements in their administrative systems, and they made a few scattered attempts to solve their economic problems (for example, Edward III tried to encourage the growth of an English woolen industry), but the net effects were small. Few people believed in 1400 that their governments were better than they had been in 1300, and a good many thought they were worse.

Nevertheless, the people who were at all conscious of the problems of their society believed that that society could be and should be saved. Naturally they did not agree on concrete proposals for reform or reconstruction. The question of priorities bothered them as it does us. "Reform of the Church in head and members" was a nice phrase, but there was no agreement on where to begin. "Tax reform" and "sound money" were popular ideas then as now, but a real tax reform unaccompanied by other changes would have ruined governments that were barely solvent as it was, and sound money is not the best remedy for deflation. It is not surprising that most efforts for reform broke down; it is surprising that interest in reform never vanished. And what is most surprising of all is that the selfish, divided, muddle-headed people of Western Europe did begin to pull out of their troubles in the fifteenth century and did make some useful and necessary modifications in their society.

This result was so surprising that historians have never agreed on how it was achieved. Most of the Grand Designs failed—for example the English ordinances of 1311, the great ordinance of 1357 in France, and the skillfully drafted constitutions of the Italian city-states. The few Grand

[12] I do not discuss technology in the next few paragraphs because I feel that it had less influence on attitudes and behavior in the fourteenth century than did the three beliefs mentioned and because there is no evidence to show whether expectations outran results in this field.

[13] In fourteenth-century France Aristotelian translations did apparently lead to greater care in the selection, or even the election, of high officials. See Françoise Autrand, "Office et officiers royaux en France sous Charles VI," *Revue Historique*, fasc. 492 (1969): 313–16; and Siméon Luce, *La France pendant la guerre de Cent Ans* (Paris, 1890), 1: 179–202. But the reform did not last much past 1413.

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Designs that worked simply created new problems. For example, the Conciliar Movement ended the Great Schism, but produced the Renaissance papacy. Many little devices succeeded, but which of the little devices were really important? Take the New Monarchies—it has always been difficult to say exactly what was new about them. Some old machinery, such as the Council, was made to work more smoothly; some old offices, such as the secretaryships, were upgraded until they became more useful than they had been before. Nevertheless, Guillaume de Nogaret would not have had much trouble in fitting himself into the court of Louis XI. The great voyages of discovery helped get the economy off dead center, but the great voyages were possible only because there had been many earlier voyages—to the Canaries, to the Gold Coast, to the Azores. And the lesser voyages in turn depended on gradual, almost imperceptible improvements in shipbuilding, seamanship, and the art of navigation. When was the turning point, what were the changes that made it possible to reach farther and farther out from the shores of Europe?

The only answers to such questions that I can find are answers based on the intangibles that I have mentioned before: hope, belief in the value of basic elements in the civilization, and a sort of unreasoning persistence. The English tinkered with their Council all during the fourteenth and early fifteenth centuries because they believed that a properly constituted Council was the key to good government. None of these experiments was very successful, and yet in the end the English produced the wonderfully effective Council of the Tudors. The early voyages of discovery added little to the wealth of Spain and Portugal, yet (though it was a near thing) the push into the Atlantic continued. Most attempts to stimulate the economy through governmental or corporative intervention failed, and yet there was an economic revival in many parts of the West in the fifteenth century. I suspect that wasting money on new weapons or in trying to send men to the antipodes was more helpful than all the well-intentioned, unenforceable, and self-defeating regulations drafted by administrators and businessmen. But even when we give all possible credit to the activities of governments and corporations, we must conclude that economic expansion depended for the most part on small increments produced by men who made the most of very slight opportunities—peasants who managed to raise a little more grain, ironmakers who managed to smelt a few hundred more pounds of iron, merchants who managed to bring in one extra shipload of goods.

Certainly the old-fashioned virtues of fortitude, persistence, and hard work played a part in the slow recovery of the fifteenth century. So did the willingness to take risks, shown by the entrepreneurs who invested in the rural textile industry, or the Baltic grain trade, or the expansion of mining operations in Germany. So did pure chance. If one proceeds by trial and error, the chances are against making all the possible errors

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before reaching one successful result. If even an earthworm can learn to find its way through a maze by trial and error, a human community ought to be able to solve some of its problems by the same method. But it is easier to persist, to take risks, to continue to try after a long series of failures if one believes in the basic values of one's civilization. It takes hundreds of trials to restore stability to a society that has been shaken by the apparent inadequacy of its ideals or by wide discrepancies between its ideals and its actual behavior. And the first successful trials may merely aggravate the problem, both because they make the unreformed sectors look even worse than they did and because human activities are so interconnected that isolated reforms cannot survive. Every significant change in society requires a host of changes in related fields, and it is a long and weary task to strike a new balance among all these factors. Naturally, during the years of testing and searching many people become discouraged and cynical. What is essential is that some people keep on trying, for if enough people keep on trying complete collapse can be averted. How large this critical mass must be is a problem that can never be solved, since quality is as important as size. In the fourteenth century the critical mass was probably less than half the population, but it had enough drive to carry Western Europe through a period of violence, depression, and uncertainty. In the fourth century the critical mass was too small and too weak to renew itself, much less its civilization, and the saving minority vanished along with the society it was trying to save. Determining what the critical mass now is, and where it can be found, is a task that I leave to my colleagues in modern history.

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Africa and the Caribbean in the Atlantic Slave Trade

RICHARD B. SHERIDAN

CHATTEL SLAVERY AND THE NEAR-SLAVERY of indentured servitude had their economic origin in the abundance of land in proportion to population. Free white settlers in the colonies were prone to disperse over the land and practice a semisubsistence economy that combined such activities as hunting, trapping, and primitive agriculture. On the other hand, the production of agricultural staples required a labor force that was numerous, concentrated, and capable of sustained effort. Since free labor could be obtained on these conditions only by the payment of high wages, it was found expedient to employ bonded or enslaved men under rigorous discipline imposed by white masters. Writing in 1849, Edward Gibbon Wakefield asserted that "every colony that has prospered, from the time of Columbus down to this day, has enjoyed in some measure what I have termed combination and constancy of labour. They enjoyed it by means of some kind of slavery."[1] According to Dr. H. J. Nieboer, it is a general rule that "only among peoples with open resources can slavery and serfdom exist, whereas free labourers dependent on wages are found among peoples with close resources."[2] The economist Ester Boserup finds a close relationship between sparse population, long forest cultivation, and the existence of slavery.[3]

Adam Smith believed that it was the experience of all ages and nations that the work done by freemen was in the end cheaper than that performed by slaves. Smith's sweeping generalization was qualified by Herman Merivale, the nineteenth-century colonial reformer, who wrote that "slave labour is dearer than free *whenever abundance of free labour can be procured.*"[4]

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[1] Edward Gibbon Wakefield, *A View of the Art of Colonization* (Oxford, 1914), 175.

[2] H. J. Nieboer, *Slavery as an Industrial System* (The Hague, 1900), 389.

[3] "Bonded labour is a characteristic feature of communities with a hierarchic structure, but surrounded by so much uncontrolled land suitable for cultivation by long-fallow methods that it is impossible to prevent the members of the lower class from finding alternative means of subsistence unless they are made personally unfree." Ester Boserup, *The Conditions of Agricultural Growth: The Economics of Agrarian Change under Population Pressure* (Chicago, 1965), 73–75.

[4] Adam Smith, *The Wealth of Nations* (Modern Library ed.; New York, 1939), 80–81; Herman Merivale, *Lectures on Colonization and Colonies* (London, 1928), 303.

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As free labor and bonded white labor became less abundant and more costly, planters turned to the cheaper labor of African slaves. They found that although slaves called for a larger original outlay of capital than did indentured servants, slaves were perpetual instead of temporary servants, they were usually cheaper to feed and clothe, and they replaced themselves to some extent by natural breeding. It was not that whites were incapable of hard labor on West India plantations, but rather that they were too few and too costly. Africa supplied labor that was generally superior to both whites and Indians in capacity, endurance, and immunity to disease.[5]

THE MOST OBVIOUS FIRST APPROACH to Afro-Caribbean history is to consider the relationship of population to physical resources in a tropical setting. Although the origin of agriculture is a topic of some controversy, it is my contention that agriculture in West Africa first developed in wooded lands where the inhabitants grew plants from cuttings in rough clearings. Low population density meant that little land was cleared and planted in a given year, although the community claimed cultivation rights to a much larger area for land rotation. Slash and burn agriculture, dependent on fertilization by wood ashes, was, in effect, a system of long forest fallow.[6]

Changes in the agricultural system were called for as the pressure of population on the resources of a given area increased. More trees had to be felled, and more time and effort devoted to land preparation, planting, weeding, and harvesting. Fire and the digging stick no longer sufficed. Rather resort was had to axes, hoes, spades, and other hand tools, and, conditions permitting, to animal-drawn ploughs. Given a rising man-land ratio, agriculture might be expected to pass through the following stages: long forest fallow, bush fallow, short fallow, and possibly annual cropping or even multicropping.[7]

[5] Eric Williams, *Capitalism and Slavery* (Chapel Hill, 1944), 19–29; Philip D. Curtin, "Epidemiology and the Slave Trade," *Political Science Quarterly*, 83 (1968): 190–216.

[6] There is an impressive literature on the part played by the West Africans in agricultural origins and dispersals. Roland Portères and George P. Murdock maintain that the cereal culture of the savanna preceded the vegeculture of the tropical rain forest, and Murdock contends that agriculture was independently developed by the Negroes of the West African savanna. See Portères, "Berceaux Agricoles Primaires sur le Continent Africain," *Journal of African History*, 3 (1962): 195–210; and Murdock, *Africa: Its Peoples and Their Culture History* (New York, 1959), 64–76. These views are criticized by J. Desmond Clarke, H. G. Baker, and W. B. Morgan in papers read at the Third Conference on African History and Archaeology, School of Oriental and African Studies, University of London, July 3–7, 1961; they are published in the *Journal of African History*, 3 (1962). Morgan concludes his paper, "The Forest and Agriculture in West Africa," with the statement: "In the light of present evidence, arguments about the origins of cultivation in West Africa must be speculative" (p. 239). Further criticism of the Murdock thesis may be found in Christopher Wrigley, "Speculations on the Economic Prehistory of Africa," *Journal of African History*, 1 (1960): 189–203. He says that very serious consideration should be given to the views of ethnobotanist Carl O. Sauer, who maintains that tropical gardening, based on the propagation of fruit and tuber plants, is ancestral to the sowing of field crops. *Agricultural Origins and Dispersals* (New York, 1952), 20–24. Although I claim no expertise in these matters, I have opted for the Sauer hypothesis, which has been developed into a model of agrarian change by Ester Boserup in *The Conditions of Agricultural Growth*.

[7] Boserup, *Conditions of Agricultural Growth*, 15–34.

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West Africa is divided into three beltlike zones that extend from east to west for great distances. Between the coast and the Sahara Desert there are the successive zones of tropical rain forest, open woodland, and grass and scrub. Professor Daryll Forde maintains that there was a general, though by no means complete, dichotomy between the forest and the savanna peoples. Before the coming of the Europeans the former appear to have been chiefly long forest fallow cultivators. They grew a variety of root crops, which were supplemented by legumes, fruits, and oil palms. They possessed small livestock—goats, pigs, and fowls—but few cattle or horses because of the tsetse fly. Iron tools, particularly hoes, were generally available but often only through trade. Labor was both individual and cooperative, depending on the tasks to be performed.[8]

By contrast with the forest peoples, those occupying the savanna zone to the north were predominantly grain cultivators and pastoralists. Their agricultural system was more intensive and their fallow period of shorter duration. Deep hoeing, ridge cultivation, and irrigation were practiced by the more advanced Hausa peoples of northern Nigeria and the Bambara and Mandingka of what is now the Republic of Mali. Savanna agriculture, moreover, may have supplied greater surpluses to maintain specialists in craft, ritual, and government, although it is uncertain whether these surpluses necessarily led to a cultural superiority over the forest peoples to the south.[9]

The shifting nature of agriculture is alluded to by Olaudah Equiano (ca. 1745–97), the Ibo slave who was brought from the interior of what is now eastern Nigeria to the West Indies and later acquired an education and purchased his freedom. "Our tillage," wrote Equiano, "is exercised in a large plain or common, some hours walk from our dwellings, and all the neighbours resort thither in a body. They use no beasts of husbandry; and their only instruments are hoes, axes, shovels, and beaks, or pointed iron to dig with."[10]

West India sugar plantations incorporated certain features of the woodland and savanna cultures of West Africa. Where land reserves were ample,

[8] Daryll Forde, "The Cultural Map of West Africa: Successive Adaptations to Tropical Forests and Grasslands," in Simon and Phoebe Ottenberg, eds., *Cultures and Societies of Africa* (New York, 1960), 116–26. Recent studies that have modified Forde's interesting pioneer work include W. B. Morgan, "The Forest and Agriculture in West Africa," *Journal of African History*, 3 (1962): 235–39; and Peter Morton-Williams, "The Influence of Habitat and Trade on the Politics of Oyo and Ashanti," in Mary Douglas and Phyllis M. Kaberry, eds., *Man in Africa: Essays Dedicated to Daryll Forde* (London, 1969), 79–98.

[9] Forde, "Cultural Map of West Africa," 126–38; E. W. Bovill, *The Golden Trade of the Moors* (2d ed.; London, 1968); Jan Vansina, *Kingdoms of the Savanna* (Madison, 1966); Oliver Davies, *West Africa before the Europeans: Archaeology and Prehistory* (London, 1967).

[10] Olaudah Equiano, *The Interesting Narrative of the Life of Olaudah Equiano, or Gustavus Vassa, The African. Written by Himself* (4th ed.; Dublin, 1791), 15. For a biographical and historical introduction to Equiano's autobiography and extracts from his book, see G. I. Jones, "Olaudah Equiano of the Niger Ibo," in Philip D. Curtin, ed., *Africa Remembered: Narratives by West Africans from the Era of the Slave Trade* (Madison, 1968), 60–98; and Equiano, *Equiano's Travels*, ed. Paul Edwards (New York, 1966).

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as in Jamaica, canes were grown under a system of bush fallow with little or no application of fertilizer. Africans, whether at home or abroad, were hoe cultivators. In both regions they commonly tilled kitchen gardens and kept small livestock in close proximity to their living quarters. As in Africa, slave provision grounds were cultivated on a shifting basis on the marginal lands of Jamaica and the Lesser Antilles. Certain food crops, such as plantains and coconuts, were common to Africa and the West Indies; others crossed the Atlantic Ocean in slave ships. Cassava, maize, groundnuts, and sweet potatoes were carried from the West Indies to Africa, while in a reverse direction moved Guinea corn, yellow yams, Kola nuts, and ackees.

Though certain similarities are evident, the agricultural systems of the two regions differed in most essentials. One was a tradition-directed social system that was geared to self-sufficiency; the other produced staples for overseas markets in response to the profit motive. One was a farm without a factory; the other called for heavy investment in processing equipment, shipping, and marketing facilities. One required few if any livestock inputs; the other employed horses, mules, and oxen for cartage, power, and fertilizer. One was essentially a medium to long fallow system; the other became so intensive in some areas that sugar canes were interplanted with provision crops. Plantations increased in acreage, labor force, and capitalized value. By the mid-1770s the median sugar plantation in Jamaica was valued at about £19,500 sterling; it contained 600 acres of land, 204 Negro slaves, and 174 head of livestock, together with sugar works and utensils.[11]

Altering the pace and timing of agricultural evolution were opportunities for emigration and aggrandizement by means of war and trade. Trade, warfare, and domestic slavery appear to have ancient roots in West Africa. Trans-Sahara travel and trade probably go back to the period of the last Ice Age in Europe when the Sahara was not desert but grassland. As the Sahara dried up, North Africans established caravan routes to West Africa, where in later centuries there emerged the great empires of Ghana, Mali, and Gao. In the centuries before their conquest by Rome in 146 B.C., the Carthaginians made occasional trading voyages along the Atlantic coast south of Morocco. The North Africans may have introduced the Negro to Asiatic luxuries and, in areas not infested with the tsetse fly, to animal husbandry. Their camel caravans returned across the Sahara from Negroland laden with slaves, gold, ivory, and a variety of tropical goods. Then, with the opening of the Age of Discovery in the fifteenth century, Europeans outflanked the Sahara by sea.[12]

Besides the impact of changing man-land ratios upon the agricultural systems, Afro-Caribbean history has been influenced by the epidemiology of

[11] Richard B. Sheridan, "The Wealth of Jamaica in the Eighteenth Century," *Economic History Review*, 2d ser., 18 (1965): 296–306.

[12] J. D. Fage, *A History of West Africa* (Cambridge, 1969), 13–17.

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the slave trade. Professor Curtin has sought to explain how the epidemiology of migration influenced the location of plantations in the American tropics and the planters' preference for Africans over other workers. He has related epidemiological theory to the period of the slave trade and the whole complex of commerce and production that made up what he terms the South Atlantic System. After discussing the complex theory of the interchange of diseases and peoples and their respective immunities and vulnerabilities, Curtin moves on to the disease environments in the pre-Columbian Atlantic basin. The different circumstances of physical environment and isolation created three distinct groups of disease environments: Europe and North Africa, tropical Africa, and the American tropics. Curtin has drawn on the statistics of military mortality in the nineteenth century to show how these environments interacted. Plantations were located in the American tropics partly because the mortality rate among Europeans was considerably less there than it was in West Africa. By infecting the Indians of the Greater Antilles, Europeans and Africans depopulated some of the best agricultural land in the tropical world. Though the imported Africans worked better and lived longer than Indians and white indentured workers, they suffered an excess of deaths over births, which made for the continuation of the slave trade.[13]

SLAVERY IN THE TWO SOCIETIES was functionally related to the agricultural systems. Whereas wholesale or gang slavery became a characteristic feature of Caribbean plantations, retail or domestic slavery was prevalent in West Africa. Apart from the Crown plantations worked by gang slaves in eighteenth-century Dahomey, the African master commonly worked with his few slaves and regarded them as inferior members of his family. Land-intensive agriculture appears to have made relatively light demands on domestic slaves. John Newton, the master of a slave vessel who later became an Anglican clergyman and antislavery leader, wrote concerning the people of what is now Sierra Leone:

> The state of slavery, among these wild barbarous people, as we esteem them, is much milder than in our colonies. For as, on the one hand, they have no land in high cultivation, like our West India plantations, and therefore no call for that excessive, unintermitted labour, which exhausts our slaves; so, on the other hand, no man is permitted to draw blood even from a slave.[14]

Modern African historians not only confirm the views of Newton, but several of them have also questioned whether domestic slavery antedated the Arab and Atlantic slave trades.[15]

[13] Curtin, "Epidemiology and the Slave Trade."

[14] John Newton, *The Journal of a Slave Trader, 1750–1754*, ed. Bernard Martin and Mark Spurrell (London, 1962), 107–08.

[15] I. A. Akinjogbin, *Dahomey and Its Neighbours 1708–1818* (Cambridge, 1967), 18–21; E. A.

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Though more strenuous than its sister institution in Africa, slavery in the infancy of the sugar industry was considerably milder than it became in a later period of intensive culture. Male and female slaves were imported in about equal proportions, family relationships and reproduction were encouraged by planters, land for provision culture was relatively abundant, and slaves were often assigned to lighter tasks than indentured servants. Both Richard Ligon and Richard Blome testified to the Barbadian planters' policy of buying male and female slaves in equal numbers. "For the increase of the stock of Negroes," wrote Blome in 1672, "they generally take as many *Men* as *Women*." Ligon, who was in Barbados from 1647 to 1650, said that the slaves were "kept and preserv'd with greater care than the servants, who . . . are put to very hard labour, ill lodging, and their dyet very sleight."[16] One case study points to low slave mortality in the early plantation history of Jamaica. The late Professor J. Harry Bennett has told the story of Cary Helyar, who had nearly completed his sugar plantation by June 1672. By that time he had purchased fifty-five Negroes. Bennett finds it surprising and significant that Cary had not lost a single slave during a period of two years and eight months. He attributes their survival chiefly to the comparatively light tasks of the slaves during the unfinished state of the plantation.[17]

West Indian slavery assumed a harsher aspect as the man-land ratio increased and the industrial-type discipline of gang slavery supplanted the extensive cultivation of plantations with a mixed labor force of whites and blacks. Slave imports tended to increase in relation to population growth, for on the one hand there was a rise in mortality and on the other hand a declining birth rate. Among the reasons for high mortality were disease, despondency, accidents, malnutrition, and the harsh labor regimen. Labor requirements increased under a system of intensive culture that called for the holeing, annual planting, dunging, and weeding of cane fields. At times dietary standards were impaired when food imports were delayed or prevented by the exigencies of war and trade. The incidence of disease increased because slave vessels were often overcrowded, unsanitary, and poorly provisioned; because debilitated slaves were set to heavy field labor before they were properly acclimated; and because contagious diseases were a function of population density and of exposure to both European and African maladies. The diseases that afflicted the slaves included dysentery,

Ayandele, "Observations on Some Social and Economic Aspects of Slavery in Pre-Colonial Northern Nigeria," *Nigerian Journal of Economic and Social Studies*, 9 (1967): 329–38; Walter Rodney, "African Slavery and Other Forms of Social Oppression on the Upper Guinea Coast in the Context of the Atlantic Slave-Trade," *Journal of African History*, 7 (1966): 431–43; Walter Rodney, *West Africa and the Atlantic Slave-Trade* (Nairobi, 1967); J. D. Fage, "Slavery and the Slave Trade in the Context of West African History," *Journal of African History*, 10 (1969): 393–404.

[16] Richard Blome, *A Description of Jamaica* (London, 1672), 86; Richard Ligon, *A True and Exact History of the Island of Barbadoes* (London, 1673), 43.

[17] J. Harry Bennett, "Cary Helyar, Merchant and Planter of Seventeenth-Century Jamaica," *William and Mary Quarterly*, 3d ser., 21 (1964): 68–69.

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yaws, smallpox, scurvy, leprosy, lockjaw, dropsy, ulcers, worms, and a variety of fevers.[18]

The Reverend Robert Robertson, a Nevis clergyman and planter, revealed in 1732 some of the reasons for the high mortality and low birth rate:

> The Loss in Slaves (not including those immediately from *Guinea,* of which about two Fifths die in the Seasoning) may well, one Year with another, be reckoned at One in Fifteen; in dry Years when Provisions of the Country Growth are scarce, I have known it One in Seven in my Plantations, and the same or worse in Sickly Seasons; and when the Small-Pox . . . happens to be imported, it is incredible what Havock it makes among the Blacks.
>
> To this the Births are to be opposed. But, considering how hard the Negroes are generally kept to work, and that Polygamy (which . . . is found . . . to hinder breeding here) is permitted to them: and allowing for the Numbers of Infants that die, the little Work the Mother can do for three Months before and nine after the Birth, Midwifery, and some other Incidents, and the Maintenance of the Child for six or seven Years at a Penny per *Diem,* the Gain from thence cannot be great.[19]

Contributing to the practice of polygamy was the declining ratio of female to male slave imports. Of the 2,269 slaves imported into Barbados by the Company of Royal Adventurers between 1663 and 1667, 46 per cent were men, 45 per cent women, 6 per cent boys, and 3 per cent girls. On the other hand, the Royal African Company delivered alive approximately 100,000 Negroes in the colonies between 1672 and 1713. An analysis of 60,000 of these slaves reveals that 51 per cent were men, 35 per cent women, 9 per cent boys, and 5 per cent girls. In Jamaica a leading slave dealer imported and sold 10,149 males and 6,145 females between the years 1764 and 1774.[20]

The evils of Barbadian slavery in 1680 were given point by the Reverend Morgan Godwyn, resident clergyman and early critic of the peculiar institution. He wrote that the planters were not very constant "to that first and so very *agreeable* Principle, of preserving these poor Wretches for Labour (their only end in purchasing of them) by a due *provision* of Food . . . and Cloathing." Godwyn was righteously outraged to find that some planters deliberately overworked and underfed their slaves to induce mortality among the superannuated, who, in turn, were replaced by young and vigorous slaves. These cold-blooded practices, which were apparently profitable under the circumstances of plentiful and cheap imported slaves

[Text resumes p. 26]

[18] Curtin, "Epidemiology and the Slave Trade," 210–15; George Roberts, *The Population of Jamaica* (Cambridge, 1957), 35–38, 221–35.

[19] Rev. Robert Robertson, *A Detection of the State and Situation of the Present Sugar Planters* (London, 1732), 42–44.

[20] Elizabeth Donnan, ed., introd. to *Documents Illustrative of the History of the Slave Trade to America* (New York, 1965), 1: 88; K. G. Davies, *The Royal African Company* (London, 1960), 299; *Two Reports from the Committee of the . . . Assembly of Jamaica . . . on the Subject of the Slave-Trade, and the Treatment of the Negroes . . .* (London, 1789), 11.

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The following illustrations are taken from William Clark, *Ten Views in the Island of Antigua, in which are represented the Process of Sugar-Making, and the Employment of the Negroes in the Field, Boiling-House, and Distillery* (London, 1823). Reproduced by courtesy of The West India Committee, London.

Holeing a Cane-Piece. Cane cuttings were planted in holes about five feet square and five or six inches deep. This strenuous labor was undertaken to check wind and water erosion, protect young cane plants from the trade winds, and to concentrate fertilizer. In the foreground is the black driver who supervised the workers wielding heavy iron hoes. To the left is the cattle pen where cane leaves and grass were mixed with dung, urine, and loose earth to make the compost for fertilizing the canes.

Planting the Sugar-Cane. Cane cuttings were laid in each hole and covered with about two inches of mold from the excavation. As the plants grew, the hole was filled with mold and the compost that the slaves carried in baskets from the cattle pen. Slaves weeded the growing canes until they were tall enough to crowd out the under-vegetation. Fort Monk is in the background.

Cutting the Sugar-Cane. Slaves are shown wielding machetes or cutlasses. Each cane was cut close to the ground and stripped of leaves and the flag-like top. Canes were bundled into faggots, tied with cane tops, and transported in oxcarts to the windmill. See the cover of this issue for the windmill where canes were crushed between three iron rollers.

Interior of a Boiling House. Cane juice was conveyed from the windmill into copper kettles in the boiling house. Slaves skimmed off impurities that rose to the surface as the juice was boiled. From the large copper to the left the juice was ladled into smaller and smaller coppers until it was reduced to a thick mass. From the last copper the mass was shoveled into trays to crystallize into brown, or muscovado, sugar. Inspecting the newly made sugar are two planters who are dressed in formal attire.

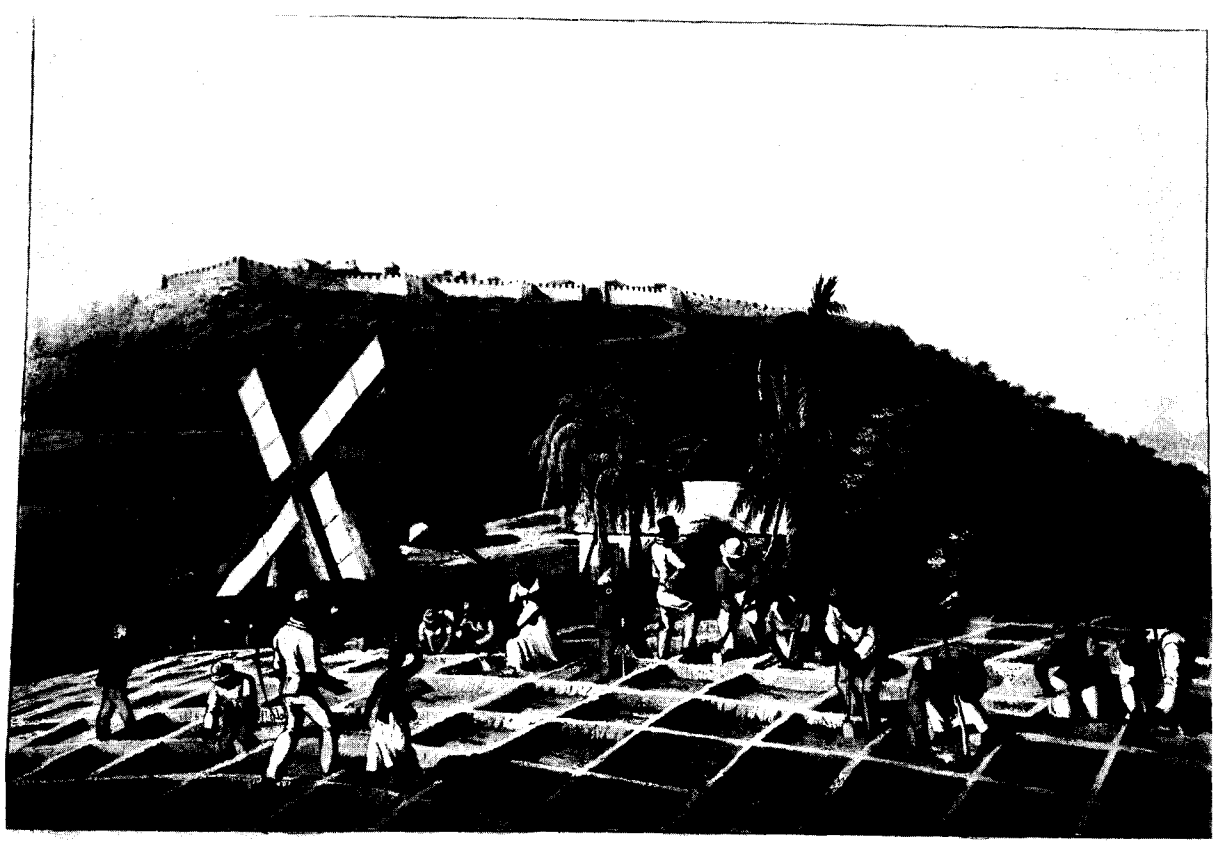
Exterior of a Distillery. The molasses or uncrystallized residue of the boilers was conveyed to the distillery where it was diluted and fermented and then poured into the copper still where the rum was made.

Shipping Sugar. Muscovado sugar was packed into hogsheads of from twelve to fifteen hundredweights and transported in oxcarts to the waterside. The slaves are seen pushing a hogshead into a small boat. They row the boat to a small craft called a lighter or drogher. After several hogsheads are loaded, the lighter takes them to an ocean-going vessel anchored in the harbor (not shown in the print).

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HOLEING A CANE-PIECE,



PLANTING THE SUGAR-CANE,

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CUTTING THE SUGAR-CANE.

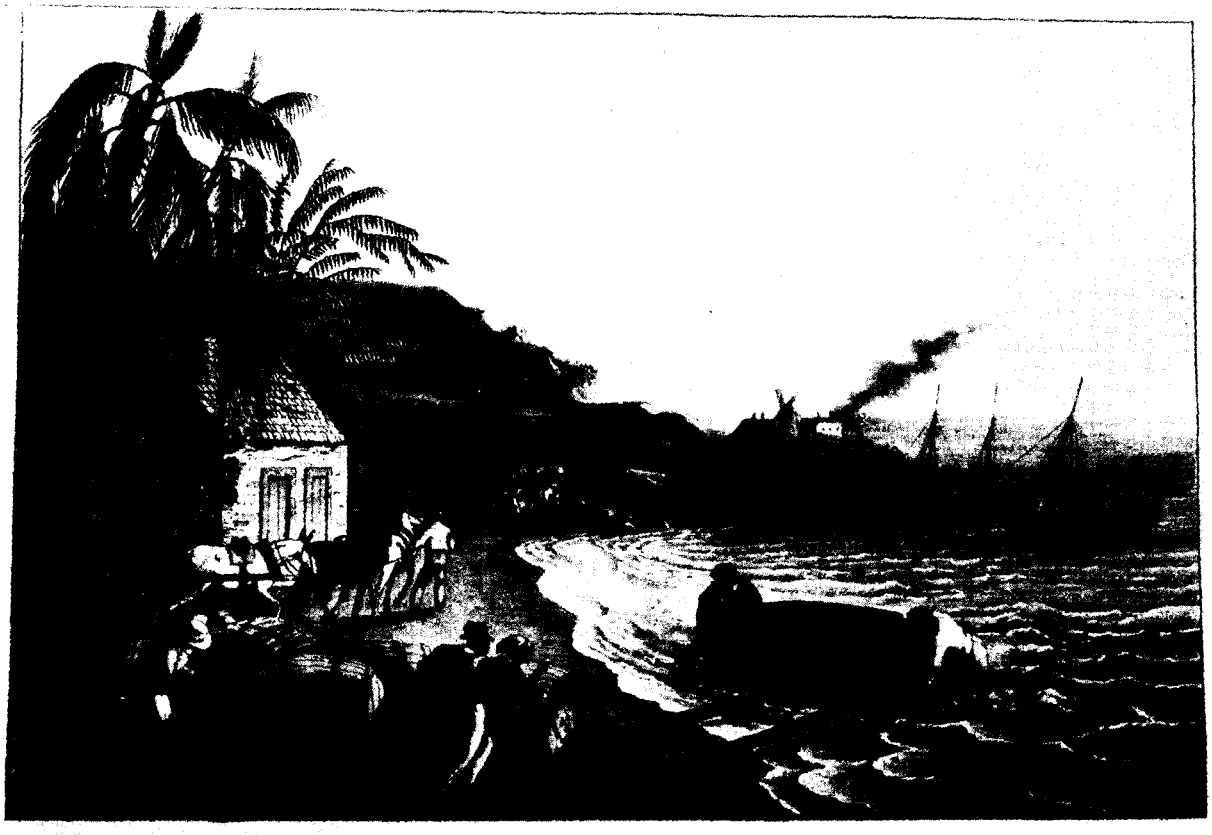


INTERIOR OF A BOILING HOUSE

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EXTERIOR OF A DISTILLERY
on Weatherills Estate - Antigua.



SHIPPING SUGAR.
Antigua on Parry's Wharf

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[Text continued from p. 21]
and costly provisions, were more common among the "richer and mightier" than among "the Middle and Meaner sort; who do usually find it less convenient to *buy new,* than (having but a few) to *preserve* their old Negro's." Great planters contributed indirectly to the re-emigration of whites from Barbados, for an effect of their scant allowance of food to their slaves was "many *Robberies* and *Thefts* committed by these starved People upon the poorer English."[21]

Numerous contemporary and modern writers have testified to the inability of the slave population to reproduce itself, with the consequent need for new slaves from Africa. Both Edmund Burke and David Hume called attention to the deplorably high mortality. "It is computed in the WEST INDIES," wrote Hume, "that a stock of slaves grow worse five per cent. every year, unless new slaves be bought to recruit them. They are not able to keep up their number, even in those warm countries, where cloaths and provisions are so easily got."[22] Professor Bennett has calculated that fourteen new slaves, or 5.46 per cent of the population of 238 slaves, were needed annually to maintain the labor force on the Codrington plantations in Barbados in the period from 1712 to 1748. A report of a committee of the Assembly of Jamaica said that "on a very low calculation, and upon a general average, sugar estates in Jamaica require an annual supply of six slaves each, to keep up the health, the strength, and the number of its labourers." Since there were approximately 200 slaves on the median Jamaican estate in the 1770s, the annual rate of natural decrease was in the neighborhood of 3 per cent.[23]

Planter indifference or opposition to breeding was, in the opinion of contemporary observers, a primary cause of the low birth rate. Sir George Younge testified that when he was in the West Indies before 1768, "the Planters did not seem desirous to encourage the Breeding of Slaves, but thought it cheaper to purchase."[24] John Newton was told by a planter in Antigua in 1751 that it was cheaper to work slaves to the utmost, and by "little relaxation, hard fare, and hard usage, to wear them out before they

[21] Rev. Morgan Godwyn, *The Negro's and Indian's Advocate* (London, 1680), 82–83. For an excellent study of Barbados in Godwyn's day, see Richard S. Dunn, "The Barbados Census of 1680: Profile of the Richest Colony in English America," *William and Mary Quarterly,* 3d ser., 26 (1969): 3–30.

[22] David Hume, "Of the Populousness of Ancient Nations" (1752), in *David Hume: Writings on Economics,* ed. Eugene Rotwein (London, 1955), 118–19; Edmund and William Burke, *An Account of The European Settlements in America* (London, 1777), 2: 124–26; Curtin, "Epidemiology and the Slave Trade," 213–16.

[23] J. Harry Bennett, *Bondsmen and Bishops: Slavery and Apprenticeship on the Codrington Plantations of Barbados, 1710–1838* (Berkeley, 1958), 61; Bryan Edwards, *The History, Civil and Commercial, of the British Colonies in the West Indies* (Dublin, 1793), 2: 467; George Roberts has computed the annual rate of natural decrease of the slave population of Jamaica at 3.7 per cent for the period 1722–34, after which it declined irregularly to 2.1 per cent in 1778. *Population of Jamaica,* 36.

[24] Testimony of Sir George Younge, May 31, 1788, Board of Trade 6/10, fols. 588–89, Public Record Office, London (hereafter PRO).

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became useless, and unable to do service; and then to buy new ones, to fill up their places."[25]

GENERALLY SPEAKING, slavery became somewhat less harsh in the decade or two preceding the American Revolution. Life expectancy appears to have risen because of two main developments. First, there is evidence that the standard of living of the slaves underwent some improvement. A salutary influence was probably exerted by such enlightened planters as Samuel Martin and Nathaniel Gilbert in Antigua, Sir Philip Gibbes in Barbados, Thomas Mills in St. Kitts, John Pinney in Nevis, and William Foster in Jamaica.[26] Nearly every substantial sugar estate had its hospital or "hot house" for slaves who needed care and isolation; sick slaves were treated by a physician in residence or one on call; minor wounds and ailments were cared for by old women slaves known as "doctresses"; pregnant women and infants were given better care; and planters had medical supplies sent out annually from England.[27]

Dr. James Grainger, a Scots physician in St. Kitts, published in 1764 *An Essay on the More Common West-India Diseases,* which included some hints on the management of Negroes. Slaves should not only be cared for when sick, he urged, but also they should be well clothed, regularly fed, and temperately punished. Every plantation should have a large sick house; nurses who were strong, sensible, and sober should be instructed to bleed, give glysters, dress fresh wounds, spread plasters, and dress ulcers; a citrus grove and herb garden should be planted; and every estate ought to be visited once a week by a doctor, and oftener if occasion required. Every planter, moreover, should keep a casebook or diary ruled into columns under the following heads: "When admitted and visited; Negroes Names; Disorders; Symptoms and Methods of Cure; When discharged; and Death."[28]

Besides improved medical care, scattered evidence points to more ample food and clothing allowances. After his second visit to the West Indies, Hector M'Neill wrote in 1788: "The treatment of slaves in these islands has certainly, within these last twenty years, undergone a wonderful change

[25] Newton, *Journal of a Slave Trader,* 112.

[26] D. W. Thoms, "Slavery in the Leeward Islands in the Mid-Eighteenth Century: A Reappraisal," *Bulletin of the Institute of Historical Research* (London), 42 (1969): 76–85; Richard B. Sheridan, "Samuel Martin, Innovating Sugar Planter of Antigua, 1750–1775," *Agricultural History,* 34 (1960): 129–31; Richard Pares, *A West-India Fortune* (London, 1950), 128–29.

[27] Richard Pares, *Merchants and Planters,* Supp. 4 of *Economic History Review* (Cambridge, 1960), 39–40; Elsa V. Goveia, *Slave Society in the British Leeward Islands at the End of the Eighteenth Century* (New Haven, 1965), 194–96.

[28] James Grainger, M.D., *An Essay on the More Common West-India Diseases; To which are added, Some Hints on the Management, &c. of Negroes* (2d ed.; Edinburgh, 1802), 88–93. For an account of a doctor who made a great impact on medicine in Jamaica, see Heinz Goerke, "The Life and Scientific Works of Dr. John Quier, Practitioner of Physic and Surgery, Jamaica: 1738–1822," *West Indian Medical Journal,* 5 (1956): 23–27. Dr. Quier, who treated the slaves on Worthy Park Estate, is cited in Michael Craton and James Walvin, *A Jamaican Plantation: The History of Worthy Park 1670–1970* (Toronto, 1970), 10, 131–33.

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for the better; but it is still far from being complete." No one denied, however, that the Negroes in Jamaica were "in *general* over-worked and under-fed, even on the mildest and best regulated properties." The improvement was attributed chiefly to the rise in slave prices that led the proprietors "to view Negro Property as an object of great concern and consequently to preserve it by every prudent method."[29]

Another reason for increased life expectancy was the growing proportion of Creole to African slaves. Since the incidence of death was generally lower among slaves born in the islands than it was among imported slaves, who had to undergo a difficult period of acclimatization, or seasoning, there came a time when the Creole population base so far exceeded annual slave imports that downward pressure was exerted on the death rate. There is some evidence, moreover, that women outlived men, with a long-run tendency toward a better balance of the sexes and possibly a rise in the birth rate. According to Governor James Seton of St. Vincent, the common duration of the slaves' lives in the 1780s was about fifty years. He explained that "this Calculation is to be confined to the Creole Negroes, or those seasoned to the Climate; for it is very certain that of the Negroes imported from the Coast of Africa, a much greater Proportion of them die under that Age than arrive to it."[30]

CALCULATIONS BASED ON STATISTICS of slave imports and slave population support the view of contemporaries who pointed to a rise in the mortality rate during the intensification of agriculture and a decline in the decades preceding the American Revolution. The data needed for computing the mortality rate are net slave imports, that is, total imports less re-exports, and the slave population at selected intervals of time. Annual imports and re-exports were recorded in Jamaica from 1702 to 1775, and imports in all but a few years from 1676 to 1701. Similarly, except for the decade 1736–46, annual imports into Barbados were recorded during the century from 1676 to 1775.[31] These two series have been adjusted where necessary to show both gross and net imports for quarter-century periods. Given the net slave imports and the slave population at the beginning and end of each period, the problem is to compute the annual mortality rate of both the Creole and the imported slaves.

[29] Hector M'Neill, *Observations on the Treatment of Negroes in the Island of Jamaica* (London, 1788), 2–5, 44.

[30] "Report of the Lords of the Committee for Trade and Plantations on the Slave Trade," *Parliamentary Papers*, 1789, vol. 26, no. 646, pt. 3, St. Vincent, A. 13; Curtin, "Epidemiology and the Slave Trade," 213–16.

[31] Slave import and population data have been compiled from numerous sources, of which the following are most comprehensive: Colonial Office 1/43, no. 37, PRO; "Report of the Lords' Committee on the Slave Trade," *PP*, 1789, vol. 26, no. 646a, pt. 3, Jamaica Appendix; *ibid.*, 1790, vol. 29, no. 697; K. G. Davies, *The Royal African Company* (London, 1957), 143, 363; Noel Deerr, *The History of Sugar* (London, 1949–50), 2: 278–79; Frank W. Pitman, *The Development of the British West Indies 1700–1763* (New Haven, 1917), 71–90, 369–92.

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The interesting thing is that while the computed mortality rates for the two islands tend to rise and fall in step with each other, they were consistently lower in Jamaica, as might be expected, than they were in Barbados. Table 1 shows the computed rates for Jamaica and Barbados, together with the estimated rates for these two islands before 1676, for the Leeward Islands from 1651 to 1775, and the Ceded Islands from 1761 to 1775.

TABLE 1. ANNUAL PERCENTAGE DECLINE IN SLAVE POPULATION†

Years	*Jamaica*	*Barbados*	*Leeward Islands*	*Ceded Islands*
1627–50		3.5*		
1651–75	2.8*	3.8*	3.5*	
1676–1700	3.0	4.1	4.0*	
1701–25	3.6	4.9	4.4*	
1726–50	3.5	3.6	4.8*	
1751–75	2.7	3.7	4.4*	4.8*

† The mortality equation adapted to a 25-year period is as follows:

$$M = \frac{I - P}{C \times A + 25 \times P_1}, \text{ where}$$

M = slave mortality rate, or annual rate of population decrease.
I = net slave imports during the 25-year period.
P = increase of slave population during the 25-year period.
C = compounding factor, or $1 + 2 + 3 + \ldots 25$, or 325.
A = annual net increase in slave population, or P/25.
P_1 = slave population in year one of the 25-year period.

For Jamaica in the period 1751–75 the solution for M is as follows when I = 177,618, P = 72,000, C = 325, A = 2,880, and P_1 = 121,000:

$$M = \frac{177{,}618 - 72{,}000}{325 \times 2{,}880 + 25 \times 121{,}000} = \frac{105{,}618}{3{,}961{,}000} = 2.7\%$$

Given the necessary census data and estimated mortality rates, the above equation can be modified so as to compute slave imports. I am indebted to my colleague Professor Duncan M. McDougall and my research assistant Mr. Lynn E. Steele for assistance in constructing this equation.

* Estimated rates.

Nearly 1,500,000 African slaves were imported into the British West Indies from 1627 to 1775, of whom about four-fifths were retained for labor in the islands. Probably another 1,500,000 British-traded slaves were carried to the thirteen colonies and foreign possessions without being entered in Caribbean port records. Distributed in time, approximately 18 per cent were imported in the seventeenth century, 19 per cent in the first quarter of the eighteenth century, 26 per cent in the second quarter, and 37 per cent in the third quarter. Barbados accounted for the greater part of the imports until the last quarter of the seventeenth century. Partly in consequence of its extensive re-export trade, Jamaica took over half the slaves

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brought to the British Caribbean in the first half of the eighteenth century. The same period also witnessed the rapid expansion of the Leeward Islands—Antigua, St. Kitts, Nevis, and Montserrat. The final quarter-century period saw the extension of the slave trade to the Ceded Islands—Grenada, Dominica, St. Vincent, and Tobago.[32]

Just how many African slaves survived the Middle Passage to labor in the mines and on the plantations of the New World is a question that has perplexed demographers and historians for two centuries. Professor Curtin's recent estimate, which is based on a careful search and evaluation of published materials, is that 9,566,100 Africans were landed alive in the New World during the more than four centuries from 1451 to 1870. Distributed in time, 274,900 (or 2.9 per cent) were imported from 1451 to 1600; 1,341,100 (or 14 per cent) from 1601 to 1700; 6,051,700 (or 63.3 per cent) from 1701 to 1810; and 1,898,400 (or 19.8 per cent) from 1811 to 1870.[33] Curtin's estimate is considerably below those of previous writers, whose totals range from about fourteen to twenty million. Using the method outlined above I have computed that 5.6 million slaves were imported in the eighteenth century, which compares favorably with Curtin's estimate of 5.4 million. Yet when one considers how widely the computed mortality rates differ from those reported by contemporaries and how large a clandestine trade in slaves was conducted, future historians may find reason to raise these estimates, which are based chiefly on the incomplete records of legal slave imports.[34]

Within the British Empire the demographic and trade patterns differed widely. Bryan Edwards, the planter-historian, estimated that 2,130,000 Negro slaves were imported "into all the British colonies of America and the West Indies" from 1680 to 1786.[35] The West Indies, both British and foreign, absorbed about seven-eighths and the Southern mainland colonies one-eighth of these laborers. Natural increase would seem to have accounted for the greater part of the slave population of the thirteen colonies, which was estimated at nearly 460,000 in 1770, as compared to 416,000 for the eleven Caribbean islands.[36]

Professor C. Vann Woodward has recently called attention to unique aspects of the slave population of the American South at the time of abolition. "Not only was it by far the largest population," he writes, "but it was derived from the smallest imports in proportion to the number emancipated or to the total black population; it was the furthest removed from African origins, and it had the longest exposure to slave discipline in large

[32] Orlando Patterson, *The Sociology of Slavery: An Analysis of the Origins, Development and Structure of Negro Slave Society in Jamaica* (London, 1967), 94–112.

[33] Philip D. Curtin, *The Atlantic Slave Trade: A Census* (Madison, 1969), 268; Fage, *History of West Africa*, 81–95.

[34] Curtin, *Atlantic Slave Trade*, 3–13.

[35] Edwards, *History, Civil and Commercial*, 2: 52–54.

[36] Curtin, *Atlantic Slave Trade*, 72–75; U.S., Department of Commerce, Bureau of Census, *Historical Statistics of the United States Colonial Times to 1957* (Washington, 1960), 756; Noel Deerr, *History of Sugar*, 2: 278–79.

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numbers." Woodward's pathbreaking comparative study concerns the slave systems of Brazil, Cuba, Jamaica, and other parts of plantation America.[37]

Contemporaries observed that while there was less suffering from cold in the West Indies by comparison with North America, the slaves in the latter colonies were better fed and their labor on the tobacco, rice, and indigo plantations was less arduous than on the sugar estates. In a unique position to judge the comparative treatment and mortality of slaves was the Reverend James Stuart, who testified before a committee of the Privy Council in 1789. Stuart, who then held a living in South Carolina, had visited the islands of Guadeloupe, St. Croix, and St. Eustatius, and had lived for eighteen months in St. Kitts. He believed that the treatment of the Negroes in America and the West Indies was "nearly alike, but in our West India Islands it appeared to him to be more severe than either the French or Danish Islands." The slaves in St. Kitts were "hardly treated, and very much oppressed by the Labour required from them." He also thought "the Punishments there very severe . . . that the Allowance of Food was too scanty." Regarding the mortality of slaves, he testified that "they do increase in Carolina, though the Climate of South Carolina is more unhealthful for the Negroes than that of our West India Islands." In confirmation of this judgment he cited three rice planters who started with a few slaves, and, by balancing the sexes and encouraging breeding, had increased their numbers from two to nine fold—adding that "more Instances of the same Nature might be given." On the other hand, he knew no instance of increase in the West Indies.[38]

BRITAIN'S LEADERSHIP IN THE SLAVE TRADE was not gained quickly or without effort or design. During the second half of the seventeenth century a foothold was established in West Africa and the flow of slaves to the colonies inaugurated by means of three Anglo-Dutch wars, the chartering of two joint-stock companies in succession, and the preference system under the Navigation Acts.[39]

Contributing to the growth of the British slave trade in the eighteenth century were such developments as the opening of the trade to independent traders after 1697, the Asiento of 1713 to supply 4,800 slaves annually to the Spanish colonies, the rise of British seapower, and Britain's growing ability to supply manufactures more cheaply, grant liberal credit, and devise cost-saving innovations in methods of trade, shipping, and finance.[40] The

[37] C. Vann Woodward, "Emancipations and Reconstructions: A Comparative Study," paper read at the Thirteenth International Congress of Historical Sciences, Aug. 16–23, 1970, in Moscow, p. 8.

[38] "Report of the Lords' Committee on the Slave Trade," *PP*, 1789, vol. 26, no. 646a, pt. 3, further evidence.

[39] Davies, *Royal African Company*, 1–19; Lawrence A. Harper, *The English Navigation Laws* (New York, 1939), 270–74.

[40] Karl Polanyi, *Dahomey and the Slave Trade: An Analysis of an Archaic Economy* (Seattle, 1966), 165–69; Richard B. Sheridan, "The Commercial and Financial Organization of the British Slave Trade, 1750–1807," *Economic History Review*, 2d ser., 11 (1958): 249–63.

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remarkable rise of the trade at the beginning of the century caught the opprobrious eye of William Wood, the mercantile writer. By contrast with the delivery of 5,155 slaves per annum by the Royal African Company from 1680 to 1688, he observed that by 1708 "there were employ'd in this *Trade* above a Hundred Ships capable of Carrying 25,000 *Negroes* a Year into the *Plantations* belonging to the *separate Traders*."[41]

British slavers forged ahead of the ubiquitous Dutchmen in the early decades of the eighteenth century, and, though challenged for a time by the French, came in some years to carry more human cargoes than the combined numbers transported by their European rivals. A report of 1753 said that British captains annually purchased 34,250 slaves, of whom 16,000 were supplied to the British colonies and the remainder to the colonies of Spain, France, and Portugal. Another report of 1768 said that European vessels carried off a total of 97,100 natives of Africa. Of this number, British vessels carried 53,100; British American, 6,300; French, 23,500; Dutch, 11,300; Portuguese, 1,700; and Danes, 1,200.[42] Table 2 shows in some detail the dimensions and nature of the trade in 1771. Since the annual net imports of the British sugar islands amounted to about 18,000 in the early 1770s, and another 4,000 slaves were carried from Africa directly to the North American colonies, it would appear that the foreign colonies absorbed most of the remaining 25,000 slaves carried in British vessels.

TABLE 2. DIMENSIONS AND NATURE OF BRITAIN'S SLAVE TRADE IN 1771

By Port of Origin			*By Source of Slaves*		
Port	*Ships*	*Negroes*	*Country*	*Ships*	*Negroes*
Liverpool	107	29,250	Bight of Benin	63	23,301
Bristol	23	8,810	Windward Coast	56	11,960
London	58	8,136	Gold Coast	29	7,525
Lancaster	4	950	Senegambia	40	3,310
Total	192	47,146	Angola	4	1,050
			Total	192	47,146

SOURCE: Bryan Edwards, *The History, Civil and Commercial, of the British Colonies in the West Indies* (Dublin, 1793), 2: 53.

Africa's ability to supply manpower at the height of the slave trade is a question that has exercised the minds of both contemporary and modern writers. "Africa," wrote John Hippisley, governor of Cape Coast Castle on the Gold Coast, "not only can continue supplying the West Indies in the quantities she has hitherto, but, if necessity required it, could spare thousands, nay, millions more, and go on doing the same to the end of

[41] William Wood, *A Survey of Trade* (London, 1718), 185–92.

[42] "Mr. Pownal's Account of the Slave Trade, 1753," in Donnan, *Documents Illustrative of the History of the Slave Trade*, 2: 507; David MacPherson, *Annals of Commerce* (Edinburgh, 1805), 3: 484.

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time."[43] T. R. Malthus concluded from his perusal of the writings on Africa and the slave trade that, notwithstanding the constant emigration, loss of life from incessant wars, and the checks to increase from vice and other causes, the population of Africa was "continually pressing against the limits of the means of subsistence."[44]

Modern historians generally support these contemporary views but admit that the slave trade may have led to depopulation in certain times and places. Professor Fage estimates that for every African who became acclimatized to plantation slavery in America, at least one other African lost his life through such aspects of the slave trade as warfare, the Middle Passage, and the seasoning. Altogether, between twenty and thirty million souls were probably lost to West Africa, which had a population that was unlikely more than twenty million at most. Since the total loss was spread over the more than three centuries of the slave trade, however, the average annual loss of less than one per cent a year of the total population "need not necessarily have been a crippling one for a healthy society." More recently Fage has scaled down his estimation of West Africa's loss of population to the Atlantic slave trade and has concluded that the effect of this loss on the population was relatively minor.[45] Basil Davidson agrees with Professor Fage that while far-reaching depopulation did not occur, at certain times and places the staggering loss and wastage undoubtedly crippled society. Dr. Boserup explains that slaves were obtained by raids among neighboring, numerically weaker tribes living by the system of long fallow.[46]

INCREASED SECURITY OF LIFE AND PROPERTY, together with some improvement in welfare and longevity, probably had a salutary influence on labor productivity. This was especially true of Jamaica, where it became safe to settle superior lands in outlying parts of the island. Edward Long, the island's historian, observed that after the accommodation with the Maroon Negroes in 1739 "settlements began to be formed in those parts of the country where none chose to venture before."[47]

According to Max Weber, slave labor has been profitable in a business sense "where it has been possible to maintain slaves very cheaply; where there has been an opportunity for regular recruitment through a well-supplied slave market; and in agricultural production on a large scale of the plantation type, or in very simple industrial processes."[48] We have seen

[43] John Hippisley, *Essays*, vol. 1: *On the Populousness of Africa* (London, 1764), 15.

[44] T. R. Malthus, *An Essay on Population* (London, 1958), 1: 91.

[45] J. D. Fage, *An Introduction to the History of West Africa* (Cambridge, 1961), 84–85; J. D. Fage, "Slavery and the Slave Trade in the Context of West African History," *Journal of African History*, 10 (1969): 398–99.

[46] Basil Davidson, *Black Mother: The Years of the African Slave Trade* (Boston, 1961), 275–77; Boserup, *Conditions of Agricultural Growth*, 73–74.

[47] Edward Long, *The History of Jamaica* (London, 1774), 1: 429.

[48] Max Weber, *The Theory of Social and Economic Organization*, tr. A. R. Henderson and Talcott Parsons (London, 1947), 253–54.

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that plantation units tended to increase in acreage, labor force, and capitalized value, and that workers were regularly recruited through a well-supplied slave market.

Slave maintenance costs consisted chiefly of food, clothing, and medical attendance. Foodstuffs were by far the largest item, but it can be demonstrated that the slaves provisioned themselves to a large extent in some of the colonies, and that relatively cheap foodstuffs and building materials were imported from North America. Though provisions were grown by slaves in all of the islands, the ratio of locally grown to imported foodstuffs was higher in Jamaica than in the small islands. As an incentive to grow provisions the Jamaican slave was given a plot of land for his own use and freed from plantation labor for one and one-half days a week including Sunday. He was allowed to plant what he wanted and was not supervised. He could specialize in one type of crop or another, raise poultry or swine, or engage in handicrafts. He could even serve as a middleman or higgler. He was generally allowed to keep any surplus he produced, to market it if he wished, and to then keep the money. Thus the policy of allowing the slave to grow food on his own land without supervision had important ramifications. It not only widened the slave's area of freedom; it also contributed to the rise of a provision economy that served as both a complement and supplement to the sugar economy.[49]

Slavery was generally profitable in a business sense despite the relatively short working-life expectancy of prime field hands. Although firm data on working-life expectancy is lacking, it can be shown that if a slave labored for twelve years he returned six per cent per annum, while fifteen years of labor yielded nine per cent, and twenty years nearly eleven per cent. These profits have been computed for slaves attached to Jamaican sugar plantations in the mid-1770s. One knowledgeable master of a slave vessel testified before the committee of the Privy Council in 1789 that much the larger proportion of the planters replaced their slaves yearly "with what they term Healthy new Negroes, rather than breed them, and look forward from Sixteen to Eighteen Years for their full and actual Service."[50]

Besides being profitable in a business sense, slavery contributed in no small way to the economic growth of the British Empire. "The *Labour* of *Negroes* is the principal *Foundation* of our *Riches* from the plantations," wrote William Wood in 1718.[51] An anonymous Englishman asserted in 1749 that "the extensive employment of our shipping in, to, and from America, the great Brood of Seamen consequent thereon, and the Daily Bread of the most considerable Part of our British Manufactures, are owing primarily to

[49] Sidney W. Mintz and Douglas Hall, "The Origins of the Jamaican Internal Marketing System," in Irving Rouse, ed., *Yale University Publications in Anthropology*, no. 57 (New Haven, 1960), 3–26; Douglas Hall, "Slaves and Slavery in the British West Indies," *Social and Economic Studies*, 11 (1962): 305–18.

[50] "Report of the Lords' Committee on the Slave Trade," *PP*, 1789, vol. 26, no. 646a, pt. 1, testimony of Captain T. Wilson.

[51] Wood, *Survey of Trade*, 179.

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the Labour of Negroes." He voiced the opinion of many of his countrymen that the Negro trade "and the natural consequences resulting from it may justly be esteemed an inexhaustible Fund of Wealth and Naval Power to this Nation."[52]

LINKS BETWEEN the economic and humanitarian aspects of West Indian slavery are admittedly tenuous. All but a few planters seem to have regarded their blacks as work units, and sentimental attachments were confined largely to domestics. But it is an oversimplification to think of West Indian society as consisting of an undifferentiated mass of black slaves and a nearly homogeneous class of white planters. The society had actually become quite complex by the eve of the American Revolution, and included slaves who had acquired craft skills and limited supervisory powers; mulattoes, both slave and free; and white merchants, money lenders, doctors, lawyers, clergymen, craftsmen, and plantation overseers. In the vanguard of the white antislavery movement were certain doctors, lawyers, clergymen, and plantation managers who, having been exposed to Enlightenment principles in Europe, were appalled by the conditions of black servitude they witnessed in the colonies. It is ironic that the planters, whose measures of slave amelioration were motivated by pecuniary considerations, should have fostered the growth of a professional class that numbered among its ranks some of the leaders of the British antislavery movement.

[52] *The National and Private Advantage of the African Trade Considered* (London, 1749), quoted in Eveline C. Martin, "The English Slave Trade and the African Settlements," in *The Cambridge History of The British Empire,* 1, ed. J. Holland Rose, A. P. Newton, and E. A. Benians (Cambridge, 1960): 437; Eric Williams, *Capitalism and Slavery* (Chapel Hill, 1944), 30–50.

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History: The Muse and Her Doctors

JACQUES BARZUN

THE SPREADING VOGUE of the term psycho-history, like much else which implies that new methods are rejuvenating history and new disciplines superseding it, must naturally raise fundamental questions of theory and practice about both history and the therapies being applied to it. Yet even a brief sampling of the current claims and congratulations will convince a fair-minded observer that these questions are being bypassed or huddled out of sight. It is therefore plausible to assume that a systematic discussion of the most important might benefit alike the students of history proper and those of the fused or fusing subject matters. One benefit would be to leave the minds in each group with clearer choices than emerge from the prevailing exhortations.[1]

At any rate, I am aware of a demand in myself and a few like-minded friends for a paper that would define and illustrate the questions in some detail, distinguish among the answers said to exemplify new genres, and point out some of the difficulties attendant upon them. Such is the object attempted in the present essay, written—as will be immediately apparent—from the point of view of a student of cultural history.

THE DISCUSSION can appropriately begin with a reminder of historical fact: the present pressing invitation issued to historians to create new fields

[1] In the spring of 1971 at least three conferences were held in the Eastern United States on the subject of psychology cum history. The largest, called together by Professor Arthur Schlesinger, Jr. at the City University of New York (Apr. 24, 1971), comprised historians (academic and other), biographers, psychologists of many tendencies, sociologists, physicians, political scientists, and publicists experienced in political life and opinion surveys. Its three main addresses and an all-day debate were refracted through the medium of the New York *Times* (Apr. 26, 1971) in a long article from which one remark served as the quotation of the day. Three months later a second *Times* article appeared on the new "tools" with which history is being revised and kept efficient (July 3, 1971). More scholarly publications of recent date, in book or article form, will be familiar to readers of this *Review* (in which some have appeared) and others will be referred to later in this essay. In this text or its footnotes, the abbreviation "Conf.," accompanied by a name, will identify a speaker or a remark made at the City University conference mentioned above. The tape of the debate is too often blurred to permit exact transcription. Throughout this article, the word history is used to mean written history(ies). For the events themselves of which history is the report, the phrase used is "the past."

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by using new methods is itself not new. When scholars native and foreign were celebrating the centenary of the Louisiana Purchase at St. Louis in 1904, the historians listened to the great Karl Lamprecht, who came from Leipzig to tell them that

> history is primarily a psycho-sociological science. . . . The new progressive and therefore aggressive point of view . . . is the socio-psychological, which may be termed modern. . . . The rise of sociology and anthropology during the last decade . . . has meant a fresh start in the writing of cultural history and in the development of method. . . . It is only the beginning of an intensive psycho-sociological method.[2]

The rest of Lamprecht's address argues that the writing of political history is out of date and that the historian's attention to great men falsifies the newly perceived social facts. His advocacy expresses an awareness of the reconstruction that had taken place in the social sciences in the nineties. Beside their youthful enthusiasm history looked old and worn; an injection of their vigor was needed, and its secret was "methods."[3]

Half a century after Lamprecht, a famous address by the outgoing president of the American Historical Association, William L. Langer, gave the label "The Next Assignment" to the proposition that history must turn to psychology, sociology, and demography in order to penetrate farther into reality. "I refer more specifically," said Professor Langer, "to the urgently needed deepening of our historical understanding through exploration of the concepts and findings of modern psychology."[4] And he illustrated possibilities by discussing the spiritual consequences of the Black Death, mentioning in passing Preserved Smith's early psychoanalytic study of Luther, a "highly neurotic" personality (1913).[5]

Clearly, the earlier suggestions and examples—Lamprecht's or Preserved Smith's—had not been fruitful. Doubtless our own mid-century psychology would be better able to provide the deeper understanding. On this last point Mr. Langer was explicit: ". . . I do not refer to classical or academic psychology which, so far as I can detect, has little bearing on historical problems, but rather to psychoanalysis and its later developments and variations as included in the terms 'dynamic' or 'depth psychology.' "[6]

One query arising from this distinction may be noted now for later use, its applicability being not to psychology alone but to all the social sciences

[2] "Historical Development and Present Character of the Science of History," in Howard J. Rogers, ed., *Congress of Arts and Science (St. Louis, 1904)* (Boston, 1905–07), 2:111.

[3] There also flourished at the turn of the century an anthroposociology, particularly associated with the name of Leon Gumplowicz. It was the latest avatar of racial anthropology, and as such embodied the never-ending search for a typology. Present-day efforts are in the main not racial, but "race thinking" takes protean forms while remaining a physico-psychological determinism. See Jacques Barzun, *Race, a Study in Superstition* (rev. ed.; New York, 1965), 158 ff.

[4] *AHR*, 68 (1957–58): 284.

[5] *Ibid.*, 296–301.

[6] *Ibid.*, 284–85. See in this connection Bruce Mazlish, ed., *Psychoanalysis and History* (2d ed.; New York, 1971); and Robert Jay Lifton, "On Psychohistory," in Herbert Bass, ed., *The State of American History* (New York, 1970), 276 ff.

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offering themselves in marriage to history. That query is, How does the historian choose among the schools, and also among their temporal phases? If Lamprecht, having his eye on current behavioral science, was right in principle in 1904, should he have waited for rightness in practice until psychoanalysis and game theory had emerged? This question affects the validity, intelligibility, and permanence of historical writing.

But before reaching this and other theoretical concerns, one more stroke must be added to the historical sketch of the "new methods" approach.[7] Psychology in its accepted meaning is the study of the mind, and its readiest application by the historian is to the individual, that is, to biography. In most discussions of psycho-history the distinction between history and biography tends to be overlooked, with unfortunate consequences.[8] One of them is to forget that between Lamprecht and Langer, in the twenties and thirties, the New Biography made a sweeping appeal with the claim that for the first time biography was truly psychological. The new genre was the subject of much theorizing, in which the stress was, of course, no longer on the great men whom Lamprecht had dismissed in 1904; the new biographies were about the same men no longer great.[9]

In the public eye Lytton Strachey was the inspired initiator, the genius who could in a few pages distill character out of indigestible masses of fact. But Strachey, as he admitted, had behind him the example of Sainte-Beuve, each of whose *Lundis* was a quintessential portrait, drawn by "a naturalist of souls"—his own self-description. Indeed, had Strachey known it, he himself had been even more closely anticipated by Gamaliel Bradford, who as early as 1895 produced a collection of what he came to call "psychographs" under the title *Types of American Character*. According to Bradford, the "graph" was dictated by nature herself, and like Sainte-Beuve he dubbed himself a naturalist of souls.[10]

In the eyes of a generation equipped with computers and adept at real, not metaphorical, graphs, all the foregoing is bound to look amateurish; but I am still discussing theory, and in theory those attempts expressed just as fully as ours the resolve to interpret, first, biography, then history by the adduction of modern psychological methods. Strachey's *Elizabeth and Essex* and innumerable other works are moldering on the shelves to prove it.

Indeed at this point a certain sense of shame should overtake us for stating in so many words and in so many places that psychology was then, or is now, about to fertilize history. Since history recounts the doings of men, it cannot go ten steps without describing character. Homer tells us that Nestor was

[7] "Approach" here is strictly used to denote the spirit of rapprochement, which is inseparable, in the endeavor here discussed, from the spirit of innovation. See below, pp. 52 ff.

[8] Pointed out by Professor Harold Syrett (Conf.).

[9] See Harry Elmer Barnes, "Psychology and History," *American Journal of Psychology*, 30 (1919): 337–76; and "Some Reflections on the Possible Service of Analytical Psychology to History," *Psychological Review*, 8 (1921): 22–37; and John A. Garraty, "The Interrelations of Psychology and Biography," *Psychological Bulletin*, 51 (1954): 569–82.

[10] *New York Post Literary Review*, 3 (1923): 641–42.

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wise, Ulysses wily, and Thersites ugly, deformed, discontented, and therefore malicious and a dissenter—the first physiopsychology of revolution. The histories (properly, the researches) of Herodotus suggest again and again that persons are not what they seem and that surmises as to motive are needed to account for their actions. The earliest great biographer in the Western world, Plutarch, was a master psychologist.[11] Autobiographers, whether the pagan Marcus Aurelius or the Christian St. Augustine, were obviously attracted to their subjects by the desire to analyze interesting characters. (Augustine is particularly good about libido and aggression in babies.) Even when, later, theology made secular history recede, the lives of the saints kept showing, side by side with miracles that were in principle uniform, characters humanly different.

Psycho-history, in short, is the latest wave in a succession of waves that began with historiography itself. Throughout we note one tendency: the psychological effort is made and remade in order to overcome the uncertainty and superficiality of history. The psycho-historian Plutarch, like his far-off disciple Langer, wants to go deeper. In his comparisons after each pair of lives Plutarch gropes toward generalization: "if only," he seems to say, "we could collect common factors and features, we would grasp the secret of the whole process." This is clearly the impulse to science, about which we shall have to inquire whether, and on what terms, it is compatible with history.

SCIENCES ARE USUALLY KNOWN by a distinctive name. It is not surprising that the term psycho-history has so soon come to prevail as that which describes divergent uses of "the new methods." It fits easily into the class of words beginning in *psycho,* of which the latest Webster lists 140—exactly twice as many as in the previous edition a third of a century ago. But the new compound can hardly be called accurate or distinguishing. It is applied, for example, to Erik Erikson's *Young Man Luther,* which is not a history but a biography; and also to studies of types or institutions, which are also not history, as we shall hear. It is set off, moreover, as if there were one "traditional" or "conventional" history from which the new is branching out, whereas there are innumerable kinds and traditions of history and virtually no conventions. As for the implication that psychology has just called on Clio for the first time, it is (as we saw) foolishness. The strongest intention in the new term is to make clear that it denotes a new discipline in the same sense as "biochemistry," or any of the later natural sciences that jointly explored border territory and compounded their names.

The facts to bear out this claim are elusive. To begin with, no single method is found in the works deemed representative of the new discipline. Even more serious, a methodical procedure is not readily apparent. The

[11] Pointed out by Theodore H. White (Conf.).

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impression given is rather that one or other of the contemporary psychologies have yielded conclusions—or more often, formulas—which the historical researcher imports into the writing up of his results. For an illustration that is convenient for its brevity, I refer again to Professor Langer's decisive address of 1958, in which he suggests a treatment of the Black Death in the light of depth psychology. In the eight pages devoted to this topic, Mr. Langer speaks of repression (of unbearable fears), unconscious guilt (due to infantile repression of sexual and aggressive drives), and of regression (to "infantile concepts") whenever man is overwhelmed by "unfathomable powers" such as great plagues. This linking of familiar facts with psychoanalytic terms is meant to help us understand the increased resort to magic, fear of witchcraft, and resurgence of religious feeling. A generality is offered: "Death-dealing epidemics like those of the late Middle Ages were bound to produce a religious revival. . . ." And, earlier: "None of the commentators . . . have traced . . . the connection between the great and constantly recurring epidemics and the state of mind of much of Europe at that time. Yet this relationship would seem to leap to the eye."[12]

To be sure, an essay of twenty pages must not be expected to deploy new methods in full; but in the hands of so able an old master it does suggest how the method would work behind the scenes and what it should perform for the audience. One easily imagines that there would be a study of documents to connect expressions of guilt more closely with the recourse to magic and religion. And any generality would be buttressed by a "control" made up of records from periods or places relatively free of plagues, marked by less witchcraft and religion.

This is a program of ordinary historical research, assuming the right evidence to be there.[13] In that research, clearly, no new method would be at work, only common sense and previous knowledge of the period. The "psycho" element therefore resides in two things: (1) the original hypothesis aiming at deeper explanation and (2) the psychoanalytic terminology for description.

Now let us suppose the search for evidence successful, that is, rewarded by abundant and clearly relevant documents, how—except for phraseology—will the written account differ from earlier ones, in which the "state of mind of the middle ages" was related to: popular ignorance, superstitious fears, the power of rumor, inadequate science, and strong institutional religion? The very words "superstitious," "ignorant," "geocentric," "terrorized," "priest-ridden" are so many psychological diagnoses. Plagues (and why not famines and wars as well?) will tend to reinforce the anxieties of

[12] "Next Assignment," 292–302.

[13] I have no competence in the field, but I happen to recall a reference to Agnolo di Tura's *Cronica*, which with its continuation spans the years 1186 to 1352, and which reports that after the great plague the survivors did nothing but feast and spend and seize the belongings of those who had died without heirs—hardly a religious revival. See Muratori's *Rerum italicarum scriptores*, XV, col. 123.

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the anxious; there is no need of depth psychology to tell us that. As for the psychological tie between calamities and the sense of guilt, it was known to Job's friends, who analyzed his situation accordingly.

Tentatively, then, we may say that the proposed treatment of the Black Death, in its hypothetically successful, documented form, would not supply a new kind of history, a sample of a new discipline, but a restatement in new language of a fairly well-known state of mind. It would surely be stretching the idea of innovation to say that a new discipline was born each time the vocabulary of psychology changed between pagan and Christian times and between deist and Marxist centuries.

This does not mean that language is unimportant, on the contrary. In what I have called the restatement, I find a disparity of level among the few technical terms that are used. It is "depth psychology" to say that guilt arises from the repression of sexual and aggressive drives in infancy. But beyond that one remark, explanation by the unconscious disappears from the account; the rest of the psychology is that of the common or garden historian speaking of fears and anxieties that arise for sufficient cause in the conscious minds of the plague-ridden populations. This is to leave the new psychological method a very narrow field indeed.

But a favorable presumption rightly obtains in behalf of whatever promises to bring more light, and in fairness one must push further. Suppose a more intensive and systematic application to history of Freudian ideas about motive. Let these ideas play upon the nature of particular medieval superstitions, upon the miracles and relics of the saints—of particular saints; or upon definite magical practices, from *sortes Virgilianae* to local rites and incantations of which records exist; and let all these be shown—if possible—in relation to identifiable fears following or preceding a particular plague. If this were done, we should all be ready to say that here was a *vera causa* of which history should take note. But after scanning the report in which that demonstration was made, we should also have to say something else. We should have to say that we had in hand, not a piece of historiography of a new kind, but a piece of psychology—or sociology or anthropology. The method of the discipline would have yielded its proper fruit: the formulation of a determinism. The novelty of the effort would lie in having established a generality with historical instead of living materials.[14]

I hasten to add that such a study would not be the less valuable on these accounts—and valuable to history. But the book—this is the important

[14] Professor John Demos, an "interdisciplinary" historian, makes the point when he says of his reliance on anthropologists: "The approach they follow differs strikingly from anything in the historical literature. Broadly speaking, the anthropological work is far more analytic, striving always to use materials on witchcraft as a set of clues or 'symptoms.'" "Underlying Themes in the Witchcraft of Seventeenth-Century New England," *AHR*, 75 (1969–70): 1312. Similarly, old Dr. Benjamin Rush was being analytic on the basis of symptoms when he wrote "An Account of the Influence of the Military and Political Events of the American Revolution Upon the Human Body," in *Medical Inquiries and Observations* (Philadelphia, 1819), 1: 125–34.

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point—the article, the monograph would not be *a work of history;* it would not be *a history of* anything.

In this last judgment I am happy to be confirmed by some of the best known of the practitioners who are generally classed under the new rubric. For example, Mr. Stanley Elkins, the author of *Slavery: A Problem in American Institutional Life,* denies that his brilliant book

> was designed as a "history" of slavery. It does not pretend to be a history, in either the extended or the limited sense. Other scholars have produced historical studies far more thorough in compass than anything I could hope to do. The present study is merely a "proposal." It proposes that certain kinds of questions be asked in future studies of the subject that have not been asked in previous ones. . . . [M]y own study has been limited, perhaps seriously, by those questions which have seemed particularly pressing to me.[15]

Nothing could be more judicious or candid, and the title of the work bears out the disclaimer. Who would wish to confuse the study of a problem in institutional life with a history? In a similar vein, on the same subject of slavery, though from a different point of view, another well-known writer declares:

> This book tries to do three complementary things: to extend an analysis of the society of the slave South presented earlier; to contribute to a rapidly unfolding discussion of the comparative study of modern slave societies; and to offer some suggestions for the development of the Marxian interpretation of history.[16]

In other words, we are promised—and given—two contributions to sociology and one to methodology. History supplies the materials (as it might to anyone—a novelist or a politician), but the result need not and should not be called a history.

In fulfilling his admirably precise program, Mr. Genovese includes a chapter on "the slaveholder's philosophy."[17] Here is a natural opportunity for psycho-history, and the author does not fail to recognize the use others have made of analytic terms for this purpose: "Guilt feelings reside deep in the individual psyche." But he concludes: "The guilt complex thesis is not necessarily wrong. it is irrelevant."[18] From which it would seem to follow that a choice still remains open to the historian not to adopt the psychology in vogue.

WHAT HAVE WE LEARNED? We now know that "psycho" prefixed to history can stand for various compoundings, which at once dispels the image of a single new discipline. We also have reason to think that the concern implied or expressed in studies bearing the new name extends over a larger tract than

[15] "Essay on Materials and Method," *Slavery* (Chicago, 1959), 224.
[16] Eugene D. Genovese, *The World the Slaveholders Made* (New York, 1969), vii.
[17] *Ibid.,* pt. 2, ch. 2.
[18] *Ibid.,* 149, 150.

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the mind. Thus Mr. Genovese offers a cultural generalization about a whole society: "Slavery did create a body of sensibilities [and] a notion of social order [but] it could not . . . produce a science or an art of its own."[19]

We can register a third point. Mr. Elkins, it will be remembered, proposed an answer to "an institutional problem." His recourse to psychology in so doing, he points out, is by way of a metaphor: "the mind of the South" is likened to the mind of a man in a concentration camp. These two hypothetical minds imagined as one are then analyzed, first according to the views of Freud; next, those of Harry Stack Sullivan; and finally those of the propounders of "role psychology." The successive analyses are elaborate; they impress the reader with the author's desire to reach positive results. Mr. Elkins is very exacting of himself, and when he thinks he has found what he sought he states it with a modest assurance that carries conviction: "We have a potentially durable link between individual psychology and the study of culture."[20]

It need hardly be pointed out that the task of expatiating on that "durable link" properly belongs to the sociologist, once the psychologist has decided what the true theory of personality is. But it is distressing to remember that someone such as Mr. Genovese may come along and say that the psychological base of the theory, though possibly right, is irrelevant. Meanwhile the "durable link" is found in an amalgam made by Mr. Elkins of Sullivan's interpersonal psychology and the later role-playing psychology. The "self," which is individual, takes us to the "role," which is a social business. Throughout the exposition, the language makes perfectly clear the nature of the enterprise we are asked to follow and accept: it is meant to be scientific.

I mean by scientific that the type of connection sought (and not alone in the book under review) implies a classificatory and thus a predictive intention. It is no accident that both the authors I have quoted admit—indeed, proclaim—the wish to find answers to pressing questions. It is a high-minded wish, and in these instances fastidious as to means and self-critical as to results. But it can without unfairness also be called a manipulating of the past in hopes of manipulating the future: if we can relate slaveholders and the ethos of the concentration camp to common determinants in infancy (or in a characteristic structure of society), we may be able to abolish the resulting evils. The search is for mechanisms; the word itself recurs.

To say that the scientific interest, charged with moral purpose, displaces

[19] *Ibid.*, 244. For other rewarding examples of psycho-sociological portraiture, see Kai T. Erikson, *Wayward Puritans: A Study in the Sociology of Deviance* (New York, 1966); John Demos, *A Little Commonwealth: Family Life in Plymouth Colony* (New York, 1970); and Winthrop D. Jordan, *White Over Black: American Attitudes toward the Negro, 1550–1812* (Chapel Hill, 1968). There is also a French *psychologie historique,* to be sampled in the writings of Alain Besançon, Ignace Meyerson, Robert Mandrou, and others.

[20] Elkins, *Slavery,* 115–23.

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the historical is not to say that historians as men and social beings feel no concern about the pressing questions of the hour and would not in any case respond warmly to a demonstration of the "durable link." In fact, at the Conference of April 1971, Professor Meyer Schapiro, the distinguished art historian, repeatedly called on the psycho-historians across the table to supply him with the durable link in a typical situation that has long puzzled him: manuscripts can be dated by the periodic change in the form of the handwriting. Why does a medieval scribe or a Renaissance lawyer's clerk "suddenly" change his hand, after which others follow suit? Mr. Schapiro received no help.

Part of the difficulty, perhaps, is to know what a satisfactory answer would be, and that is no doubt why the scientific search continues with as many ologies as give signs of seriousness and plausibility.

But to round out the present sampling of new possibilities we must ask a final question about the most obvious field for psycho-history. While the new methods struggle for an answer to the difficult problem of the passage from self to society, have they not already gone a great way to solve the enigmas of individual character? Certainly the finest achievements so far credited to psycho-history are biographies, chief among them Erik Erikson's *Young Man Luther* and David Donald's two-volume life of Charles Sumner. And different as these two works are, there can be no mistaking them for attempts at science or general formulas; they are simply lives in the modern mode. Both books are praiseworthy for reasons that invite the critic himself to historicize and psychologize: Mr. Erikson, a psychoanalyst, took pains to become a good historical researcher; and Mr. Donald, a historian, took pains to become a careful student of psychological doctrine. Add to this double preparation judgment and literary skill and you have two admirable works.

But the very fact that we recognize them as classic biographies establishes the truth that they do not constitute a new genre. To be sure, the selection of facts and the ideas that bind them in explanatory ways are not of a kind found in every biography. It can even be agreed that the importance given to psychoanalytic considerations in these works, not written for specialists (for whose purposes full case histories would be required), is a pioneering effort that marks an epoch. But the issue does not lie in these undoubted propositions. It lies in the question whether the special attitude or special capacity of a biographer creates a new discipline when he applies them.

The "point of view" has always played a great part in biography, if only because the attraction of the subject for the writer often derives from the presence of materials suited to the exercise of a special interest: the soldier writes about Napoleon as military commander; the doctor as an obese pyknic with Malta fever. Technical and doctrinal specialization in biography had already gone far before the claims of psycho-history were voiced. Many determinisms had been tried: medical, phrenological, racial, sexual, optometric, genetic, economic, criminal, theological; and even where ex-

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planation was not pushed to the point of determinism, there was—to use Mr. Elkins's words—a clear conceptual framework for personality analysis.

To allude to these precedents, some of which are obsolete, infamous, or absurd, is not to cast aspersions on present-day psychobiography. It is but to take the historical position and remind ourselves that the single-minded use of special knowledge will certainly change the atmosphere of any story, not only through vocabulary, but also by concentrating attention on certain facts at the expense of others. This shift, though it often makes "viewy" biography short-lived, cannot be said to change the "discipline" to which biography and history belong.

HAVING LOOKED INTO some of the intentions and deliverances of psycho-history on its home ground, I now pass to the larger domain of historical inquiry. The readers of psycho-history, and especially the historians among them, discover as a result of the insistence on "deeper explanation" a problem as difficult as it is neglected, for it appears to be no problem at all: What is explanation? At what point do we understand? How do we know that we do?

For the promoters of psycho-history it is a matter of course that the application of their methods "adds to our understanding." They take the view, current in our world of science, that one cannot know too much about any subject of interest and that every addition explains more. And by an unwitting play on words, the mention of "depth psychology" promises a deeper explanation. The thought-cliché is that what is most fundamental, most underlying is automatically most enlightening. This is a questionable impression. At that rate, since chemical molecules and the particles of physics lie deeper, lower, than the Id, we should be still better off with a chemo-history and a nuclear biography.

This is not said in the spirit of fantasy but of emphasis. The branch of psychology known as psycho-acoustics has made amazing progress in reducing perception (sensory phenomena) to equivalence with energy fluctuations in the stimulus: "The net outcome is that the listener can be painted out of the picture in the ideal way that sensory psychologists strive to discover." Professor McGill's achievement consists in having shown that "a mathematically ideal observer and a simple energy detector, although worlds apart conceptually, behave rather alike."[21] If, then, psycho-historians really want to push explanations in "the ideal way" of deterministic science, they should accept a mathematically accurate model or machine as equivalents of man and society and consider these permanently "painted out of the picture."

Explanation, in other words, is not an absolute term, any more than

[21] William J. McGill, "Neural Counting Mechanisms and Energy Detection in Audition," *Journal of Mathematical Psychology*, 3 (1967): 351, 361.

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understanding is an experience capable of exact specification. The historian is alive to this truth when he takes care to avoid the genetic fallacy. In reading psycho-history one often has the sense that this fallacy is being committed (and in a strangely halfhearted way) through pure verbalization. This defect was to be expected from the practice noted earlier of juxtaposing clinical terms and commonsense terms to explain one situation. It suggests, though it does not intend, the game of expert versus laity. The habit might be called "genetic tautology"; its standard illustration is Molière's "dormitive power of opium."

But this verbal fault is mixed with a more serious confusion. Everybody knows that it was by organizing and pondering the multitudinous phenomena of mental disturbance that Freud and his predecessors and successors were able to effect cures. For clinical practice it is necessary to subsume a wide range of manifestations under a small number of hypothetical causes. To the extent that therapy succeeds, it is clear that the words and phrases for the mechanisms and entities trigger the type of understanding required for action. But as both Freud and his deviating disciples showed, it is also important to keep revising, reversing, and adding to the stock of concepts. The mass of published cases, the bulk of theorizing testify to the arduousness and uncertainty of the enterprise, which is far from concluded.

Now history—as I shall show more fully later—is not for diagnosis; it does not proffer a technical analysis. It is addressed to the common reader, whose "understanding" in both senses is governed by a far subtler use of words than the technical. The signs and intimations to which he responds are more numerous and more free. And he neither can nor should have to go into training before taking up a history. His curiosity about the past should not commit him to a siege of *hineinstudieren* into a doctrine, much less to weighing pros and cons among the dubieties and contradictions of rival systems. He wants to read and learn. His understanding, his sense that something has been explained, derives entirely from what William James called "the sentiment of rationality," the impression that familiar and unfamiliar elements have been put into intelligible relation by someone who gives tokens of trustworthiness. That is intelligible to him which he finds sufficiently congruent with his experience (direct or vicarious) to make him accept the neighboring strangeness and integrate it into a new imaginative experience. For this purpose the explanatory force of common speech cannot be bettered. A short biographical example from seventeenth-century English life will perhaps make plain the adequacy of the non-technical.

Charles Mohun, born in 1674 of noble parents (his mother was herself the daughter of the Earl of Anglesey, while his father was the third of his line) was conceived under a baleful star, and violence was in his blood. While the child was still only a year old the father, a gallant and a blade in the very "best" tradi-

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tion, was killed while seconding the Earl of Cavendish in a duel with Lord Power. . . .

The widowed Baroness, inconsolable in his loss, turned all her love upon her baby son and with steady, insistent spoiling prepared him surely for the same bloody end that had destroyed his father. But that was far in the future. Meantime the lad grew from a precocious child, surrounded constantly by obedient women, into an imperious, dominating youth. Handsome, charming when need be; forthright and frank in speech and manner, almost ungovernable when thwarted; suffocating under the artificial petticoat life around him—and rich.

This combination of frustration and wealth has been disastrous to better men than Mohun, both before his time and since, but with him it was to prove fatal. Growing personal unfulfilment, the glamorous example of his swaggering father, the cloying affection of his mother and above all the awareness that Life was there for the mere taking—all these things simmered and smouldered inside him.

Yet there was much in him that was good—perhaps only a certain moral strength was lacking. He was the friend of anyone who wanted his friendship. He was a good listener to the troubles of others, and a ready sympathiser. In the physical sense he was fearless. Above all, he was utterly loyal both to his friends and their causes. And it is ironical, but true, that his good qualities led him more into disaster than his bad ones ever did. For as a result, perhaps, of his stifled upbringing on the family estates in Devon he possessed an inexhaustible capacity for picking worthless friends. . . .

He soon found himself in bad company. Captain Richard Hill of the Dragoons, rising thirty, cynical, fashionable and tough, noted ruffler and cut-throat, had but two passions in life; horses and women. Horses, he not only loved but respected; women—easier to come by than a thorough-bred and cheaper to keep—he loved in endless procession and despised accordingly. It made him irresistible, of course. Mohun became devoted to him.[22]

The upshot of this friendship was an ignoble and premature death for Mohun, after two trials for murder before the House of Lords. Now the question for anyone with a certain experience of life is whether on reading the description he understands Lord Mohun's character. The question is not, Can he "account for it"? in the sense of why it had to be so and not otherwise; but rather, does the explanation given make us visualize, sympathize, regret? Surely that is how we understand our family and friends. We also compare; we remember young men rather like Lord Mohun, ruined by fatherless, matriarchal influence. We remember besides —this is most important—other boys reared by doting mothers and aunts who did not turn out dauntless and depraved but, possibly, timid and strait-laced. There are infinite varieties of character *and circumstance*.[23]

Suppose, however, that the psycho-portraitist adds to the plain sketch the information that Mohun's recklessness was but overcompensation for a girlish, overprotected childhood; or that, oedipally in love with his

[22] Philip Marsden, *In Peril before Parliament* (New York, 1965), 64–65.

[23] Mohun's first murder was willful, but accidental in the sense that the timing of unrelated events permitted his brashness to take effect. Another imaginable youth, of the same psychic make-up but apprenticed to a tailor, would probably not even have known how to wield a sword.

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mother, he was acting the father role; or even that his known "loyalty" to young men friends was unconscious homosexuality, balanced (and perhaps also symbolized) by impulsive sword-play; or any other psychologizing with the aid of the unconscious that a competent analyst (whose words I am simulating without knowledge or authority) would deem appropriate. How would such additions in technical words enlarge our understanding? What more would they explain, not about the usual workings of the unconscious, but about Charles Mohun?

I submit, with no prejudice against that type of information, that it would not increase knowledge, would not explain further, because it would not really be about the man but about his situation, considered as typical. In those similarly burdened but who survived beyond Mohun's span of years notable changes may well have occurred. An explanation by childhood determinants seems to leave no room, in biography at least, for the *development* of character.[24]

THE DEMANDS of the common reader do not stop here. There are further questions, which at the Conference were neatly grouped by Mr. Herbert Feis: "How adequate psychological knowledge is, how stable it is, how precise it is, and how agreed it is." As to stability, competent judges apparently differ. The theorists of psycho-history invoke Freud and speak of psychoanalysis or of dynamic psychiatry as if those names covered unified teachings. Yet even a slight acquaintance with the literature discloses radical conflicts subsisting and indeed ramifying among interpreters of the unconscious. In the interesting writings of Dr. Thomas Szasz even the classic relation of patient to analyst is disputed and the very notion of mental illness put out of court.[25] The rifts began with the first great disciples of Freud, so that in the so-called depth- or dynamic psychology we have in fact a cultural amalgam in which Freudian elements may be more numerous but not less important than those borrowed from Adler, Jung, and others. Psycho-historians rarely keep the "conceptual frameworks" of the several schools as distinct as they are in the pages I cited from Mr. Elkins. But without strictness along some distinct line, where are method and the scientific force to compel belief? This neglect would seem to go to Mr. Feis's point about precision.

[24] To take an actual case, Rutherford B. Hayes was reared and oversheltered by his mother, his sister, and a maiden cousin-aunt. His biographer, Harry Barnard, says of him: "Neighbors . . . considered him as 'timid as a girl.'" But "other influences . . . set up in him a defensive, countering trend toward manliness." (*Rutherford B. Hayes and His America* [Indianapolis, 1954], 74, 81). And after action in the Civil War, Hayes became a strong, self-confident person; "he had become," says Mr. Barnard somewhat curiously, "his own father symbol." From which we infer that early conditioning, conscious or unconscious, is modifiable, or rather, that manifestations evolve; and if so, the uncertainty of arguing backwards from manifestations to essential character increases.

[25] See *The Myth of Mental Illness* (New York, 1961), and *The Ethics of Psychoanalysis* (New York, 1965).

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As to adequacy, the tangential observation must be made that the cogency of analytic thought has been diffused through long popularization until certain ideas and terms have become readily understood by the public, regardless of their doctrinal origins or exact meaning. This diffusion is what happens to all great bodies of thought. The words complex, introvert, extravert, fixation, repression, guilt, anxiety, ego, id, superego, oedipal, hysterical, father-figure, accident-prone, inferiority, manic-depressive, death wish, wish fulfillment, schizophrenic, paranoid, and others less hackneyed have replaced older terms and now occur in common conversation, hence mark a spot in the mental geography of common speakers. The terms come from successive layers of the same doctrine and from different doctrines, yet they remain indicative without being exact or consistent. Thus when Mr. Barnard, speaking of Rutherford Hayes, refers to "the fixations in which Fanny [his sister] and Sardis [his uncle] were the chief figures," we are to understand by *fixations* "deep attachments." For these two emotional relations were of very different intensities: Hayes experienced no tension or pain or marital doubts on account of his uncle as he did on account of his sister. The very plural "fixations" shows that the word is being used without technical corollaries.

This usage by way of the diffusion of ideas is as it should be; the example illustrates a principle: in order to be of use to the historian or biographer, psychological or other technical terms must have fallen into the common domain. Thus in the sketch about young Mohun, the writer says he was "conceived under a baleful star and violence was in his blood." Here astrology (genethlialogical) and a physical psychology are combined in common images, the reader's native and only idiom. Must we then, asked Professor Dankwart Rustow at the Conference, take our psychoanalytic terms from the butcher and the baker? To which the answer of any student of history who recognizes the great historians as setting the standard is: Yes. Barring a very occasional digression into semantics for certain subjects, the reader must be addressed in words intelligible to the ordinary educated man, particularly if the writer professes not only to narrate and interpret but also to explain and *give understanding*.

It could be argued that if psychological terms are not to be used with technical precision, they might as well be omitted altogether—why "fixations" when we can say "deep attachments"? I am not pressing this point; a writer is entitled to any words he thinks will convey his meaning, and those chosen are bound to vary with the times. Our ambient puts a premium on terms suggestive of science. Where an earlier psychology spoke of "conviction of sin," "state of orison," or "counter-conversion," we speak of anxiety, counter-transference, etc., to say nothing of the Marxist names for approved or sinful states of mind. All these are not, indeed, equivalents. They are signposts within diverse conceptual frameworks and alike subject to uncertainty, argument, and misuse.

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That fact leads naturally to the last part of Mr. Feis's four-barrelled question: agreement. Why is psychoanalysis in its broadest sense the only psychology favored by the psycho-historian? Could it be because its vocabulary and ideas alone have gained currency after a century of popularization? Certainly there is no warrant for believing that only psychoanalysis can show results in the study of personality. The latest edition of Woodworth's classic *Contemporary Schools of Psychology*[26] lists seven broad tendencies long pursued and still in high repute. And since the publication of that survey, the phenomenologist psychology as practiced at the Copenhagen Laboratorium has produced new findings that are now receiving respectful attention from American leaders of older schools.[27] Is it impossible to conceive that any or all of these eight kinds of teaching have something to say about human character, motives, and actions?

What *is* impossible to conceive is that a reader could encompass a paragraph, relevant to history though it is, that began: "Action potentials are to a large extent exercising an influence on my set when I perceive the behavior of another person and thereby influence what *sens* I shall perceive in a given material sequence. . . . We see how a perceived stamp creates a set so that behavior is experience . . . without any real *sens*."[28] Not until *sens*, "set," "stamp," and "action potentials" are household words will this valuable remark help "explain" (as in fact it does) why Woodrow Wilson and Clemenceau misunderstood each other at the Paris Conference. The fortunate fact is that explanation by scientific formula from any school of psychology is not indispensable to the historian. He has the means of *showing* Wilson and Clemenceau addressing each other out of their discrete fogs.[29]

Historian and biographer can indeed profit from the reading of such psychologies and pathologies as they can understand.[30] Striking case his-

[26] By Robert S. Woodworth and Mary R. Sheehan (3d ed.; New York, 1964).

[27] Franz From, *Perception of Other People*, with a foreword by Henry A. Murray (New York, 1971).

[28] *Ibid.*, 148–49. An alternative still lower on the scale of concreteness would be to account for confusion at a conference by the explanation that "the empirical masking function for sinusoids in white noise is governed by fixed signal-to-noise ratios." McGill, "Neural Counting Mechanisms," 369–70.

[29] Description—let it be said again—does not exclude the findings of special studies, provided these are intelligibly linked to the historical man or event. For example, Woodrow Wilson's neurological history as far back as 1896 is retold and analyzed by Dr. Edwin A. Weinstein in an article that does not substitute medicine for history but dovetails the two in masterly fashion; it is a model of the "special study" that any historian is happy to find and grateful for being able to use. "Woodrow Wilson's Neurological Illness," *Journal of American History*, 57 (1970): 324–51.

[30] Dr. William B. Ober, for example, has written entrancing "medical lives" of Keats and Boswell, and his "Thomas Shadwell" is a model of insight and knowledge effecting a solid rehabilitation of the poet damned by Dryden. "Thomas Shadwell: His Exitus Revis'd," *Annals of Internal Medicine*, 74 (1971): 126–30. Even the egregious Dr. George M. Gould, whose *Biographic Clinics* (Philadelphia, 1903–09) discovered ophthalmic astigmatism as the nearly ubiquitous cause and curse of genius, makes some interesting observations. Similarly, the work of W. H. Sheldon on body types, though never generally accepted, draws attention to relationships that could supply evocative words in historical portraiture.

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tories may dispel prejudice, physical and physiological connections are suggestive, definitions of terms from divergent points of view reinforce the sense of complexity, the very word *Gestalt* affects one's own consciousness of perception. I would therefore enter a caveat against the statement that classical and academic psychology have nothing to teach the historian: all reputable psychologies put him in touch with some aspect of the mind, though none furnishes him with a key to its mystery.[31] And some schools, obviously, provide a vocabulary whose currency makes it usable as a shorthand of suggestive interpretation. But if, as we saw earlier, the "guilt-complex thesis" can be right but irrelevant for Mr. Genovese in his study of one situation, it can be irrelevant for all historians who so decide, about any situation.

The principle of suggestiveness, used above to characterize "connection" and "interpretation," now directs our thoughts to the greatest weakness of psycho-history as the unhyphened historian sees it: its handling of evidence. There is no need on this point to question the logical status of psychoanalytic explanations. As everybody knows, some are tenable hypotheses; others are circular arguments. Let them be equally accepted heuristically and consider only how they impinge upon historical judgments. I believe that they may do so only in the guise of suggestion, speculation, interesting possibility, because their evidential value is of a different order from the documentary. I must stress that this is the status I gladly accord them in *history;* what I say implies nothing about their status in psychology or therapy. An example, again drawn from the Conference, may help make my point.

After a cautious and modest rehearsing of the way in which analysts form inductive generalities from repeated observations of their patients, Dr. Douglas Bond, of the Case Western Reserve University Medical School, illustrated the sort of diagnostic power thus afforded by telling an anecdote about a friend who was interested in writing the life of Rousseau. This interest led to the friend's showing Dr. Bond the famous Venetian chapter in the *Confessions* (bk. 7), where Rousseau is discomfited in front of a beautiful girl whom he passionately desired, because (says Rousseau) she was marred by the congenital lack of one nipple. The analyst then remembered reading that Rousseau suffered from enuresis as well as from a sexual malformation (hypospadias). The analyst at once diagnosed the cause of the Venetian fiasco: Rousseau by identification projected his defect upon the girl.[32]

Rousseau gives a rather different explanation, rooted in feelings which he experienced some time before he ever saw the girl's breast and

[31] The classic of psychology perhaps most useful to the historian and biographer is William James's *Principles of Psychology* (New York, 1890), particularly on such topics as instinct, motive, will, and the stream of consciousness, where his concreteness defies obsolescence.

[32] The informality of Dr. Bond's impromptu remarks does not disqualify them for discussion here, nor do I intend any derogation of Dr. Bond, who was extremely lucid and direct. The example is simply convenient as showing psycho-history as it shapes the opinions of a trained and thoughtful mind—psycho-history in its existential state.

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which its sight revived. Now the fact of his deformity is known to us independently, from ocular evidence on record. But if the autobiographical account is to be questioned, what the historian by training and temperament wants to know is whether the girl had one nipple or two—and for this also he wants independent evidence. He is not content to accept as proved fact the *possibility* that Rousseau "projected" and saw single where he should have seen double.

At times the historian must perforce be content with a likelihood, and he states it as such. But in the absence of evidence he does not equate particular facts and simple inferences from similar situations. He can always think of alternatives: since Rousseau was by no means uniformly impotent, the failure told by himself could have resulted from many circumstances other than those a psychoanalyst might think virtually certain. Human actions—or inactions—depend on a rich mixture of motives; it is one of the functions of history to keep reminding us.

Need one add that when a historian or biographer turns a plausible inference into a suggestive touch, he must be careful not to let it work upon his mind or the reader's until surmise turns into "fact"? The *locus classicus* of this type of *Aberglaube* is the discussion by F. W. Bateson of Dorothy Wordsworth's affection for her brother William, where suspicion of incestuous love grows into affirmation on no additional evidence.[33]

I am far from saying that psycho-history as a genre is bound to fall into such grievous errors. Many of its practitioners are trained historians and have shown their professional competence. But their aim in attempting to create "a new discipline" is precisely to mingle the type of evidence predicated in their training with another type derived from clinical practice. This second sort they generally take at second hand, and since it is diagnostic and formulary, it often affects the larger conclusions and not simply the isolated event. In any case, this secondary evidence is necessarily scant. Diaries, letters, literary works record moods more often than actions, and "dream-material" is extremely rare. Compared to the volume of data elicited under therapy and consciously directed toward completeness by the analyst, this filtering of written remains is almost negligible.

Moreover, the historian's materials being static, they are frequently ambiguous. The inevitable result is for the psychologizer to make the most of single points, instead of following the preferred historical practice, which is, like that of the law, to gather as many concurrent witnesses as possible. And not only many, but literal ones. At the opposite from this cautiousness the testimony of psychoanalysis is heavily metaphorical, and the habit of thought it encourages is symbolic and analogical—the

[33] F. W. Bateson, *Wordsworth, a Re-interpretation* (1st ed.; London, 1954). See also the facility with which it is assumed that Carlyle's "verbal explosions of sadism . . . were the means by which [he] obtained relief from and compensation for the misery of sexual impotence." Leonard Woolf, *Quack, Quack* (London, 1935), 122. Who knows anything positive about Carlyle's "impotence"?

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antithesis of evidential. Mr. Elkins, in the book cited, is keenly aware of the danger and he devotes a few pages to defending "analogy as evidence." His ground is that metaphor is a recognized means of communication, which is true. But it is not always true that the reader will limit the likenesses strictly to the common features in the things compared.[34] He will do so, perhaps, where the metaphor is clearly seen for what it is, but in analytic language as in any other, the image is soon taken for the thing, at which point the problem of evidence reappears.

So far the historian's concern for evidence has implied outer sources of fact. There is the inward side, or what might be called the jury's side. One might improvise about it a "psychology of Credibility." "What proves what" was a difficult subject even before psycho-history was invented, and the query is particularly troublesome in biography, where the vividness of a report or the repetition of an act may give a totally false impression of motive and character. That is why the criminal law carefully excludes certain references to character and is content to proceed without proof of motive.[35] But historians and biographers are not only paid to supply this double want (that is what is meant by interpretation), but they also find that they cannot shape intelligible patterns without it. Still historians must therefore borrow a little of the law's caution and avoid stressing certain traits for which there is evidence, lest the figure as a whole suffer from imbalance in the reader's impressionable mind. Plutarch knew of this danger (see his *Cimon*) and Harold Nicolson stressed it when he said somewhere that in writing of Curzon he played down his miserly habits and ungovernable temper, for fear nobody should remember anything else.

The bearing of this ancient wisdom is that interpretations essentially analogical may easily end up untruthful, even when the psychoanalytical reasoning seems clear.[36] This is but another way of saying that feeling is an important part of thinking and understanding. Since the reader feels

[34] *Slavery*, 224–26. For example, it has become usual (Mr. Langer, Conf.) to speak of great conquerors, the scourges of mankind, as "pathological." On what grounds? Statistically and functionally in history they seem the norm. What the metaphor "pathological" expresses is our disapproval, which I share. But pathology would tend to diminish responsibility: which way do we want it?

[35] Edward Marshall Hall: "It is always dangerous to impute motives to any man in regard to anything he says or does." From a letter quoted in the life of Hall by Edward Marjoribanks (London, 1929), 203. Sir Sydney Smith: "No doubt there is a motive behind all our actions, and few if any sane persons act without a reason. It would be wrong, however, to assume that the reason can always be appreciated by another person." *Mostly Murder* (London, 1959), 260.

[36] Freud's famous "Leonardo" essay is frequently instanced as a model of what psychoanalytic interpretation can bring out that nobody else had seen. But what it brings out is either slight or doubtful: (1) that Leonardo was a homosexual because his father abandoned him to his mother and thus fostered an unfortunate relationship. (But the father's abandonment has been disputed and one diagnostic incident has been shown wrong, owing to a mistranslation of the key Italian word.) And (2) that Leonardo's habits as an artist (not finishing work, etc.) derive from this now disputed sexual development. Here determinism breaks down at the first contrary instance, e.g. Goethe, who also found finishing difficult, though he grew up with two parents and was a fairly active heterosexual. In other writings, Freud's analyses of art and artists are distressingly jejune, though he was himself a sensitive judge of art and wrote magnificently about Dostoevsky. On the "Leonardo," see Henri F. Ellenberger, *The Discovery of the Unconscious* (New York, 1970), 530–31.

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at every moment, even when he thinks he is only taking in information, the writer must continually watch connotative tone, remembering that in our day the tone of "depth" is the technical tone, which right or wrong carries the conviction of superior truth.

An added reason for our susceptibility is that there is in all of us a hankering after the startling little cause, the tiny detail that speaks volumes. We share the not altogether admirable love of unmasking. It was this foible that Lytton Strachey exploited in his lives of *Eminent Victorians,* making his portraits doubly titillating by choosing as the telltale item something not only small but mean. He thought he was eradicating Victorian ways of thought,[37] but in fact one of the great Victorian attitudes was little different from his: Huxley and his scientific friends thrived on telling the public that its Bible and its hopes were absurd, since the sun was cooling fast. Here were the original debunkers, who dulled their own pain in causing that of others. It is relevant but not invidious to note that science is peculiarly the domain in which a small fact fully ascertained can destroy a whole edifice of thought.

THROUGHOUT THIS ESSAY I have shown or implied the kind of incompatibility I believe to exist between history and psycho-history. The one is not a desirable progression out of the other: they are different undertakings altogether. What is called psycho-history is one of a whole class of efforts, only some of which have names—socio-history, quantitative history, interdisciplinary history. Their common intention is analytic, generalizing, formulary in the spirit of science. They can all be recognized by their frequent mention of "new tools" being used to create "new disciplines." Indeed, a certain use of the word "method" suffices to disclose their difference from history properly so called.

Other identifying marks of that difference now claim our attention; for the excitement about new hybrid genres has inevitably confused both laymen and professionals as to the spirit of history. The difference itself is fundamentally that between two orientations of the human mind, the intuitive and the scientific. Pascal, who possessed the genius for both, gave of them a definitive account in his *Pensées.* Whoever wants to understand the difference, say, between Carlyle's *French Revolution* and Crane Brinton's *Jacobins,* should recall what Pascal says about the *esprit de finesse* and the *esprit de géométrie.*[38] A compressed paraphrase could run thus: in science (the geometrical mind), the elements and definitions are

[37] A reminder is needed that the salutary break with Victorian ideas of biography was made not by Strachey in 1918, but by a genuine historian, Anthony Froude, in his *Thomas Carlyle* (1882). The fact is best proved by the storm of obloquy that descended on the biographer in consequence.

[38] *Pensées,* ch. 1. It may be noted in passing about Crane Brinton's book of 1930 that its subtitle was "An Essay in the New History."

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clear, abstract, and unchangeable, but stand outside the ordinary ways of thought and speech. It is easy to use them right in reasoning once one has grasped them thoroughly; it is then but the application of a *method.* In the opposite realm of intuitive thought (*finesse*), the elements come out of the common stock and are known by common names, but it is hard to reason justly with them because they are so numerous, mixed, and confusing. What is needed there in a high degree is a discerning eye and superior judgment, for there is *no method.* In short, the *esprit de finesse* deals with "indivisibles." Neither *esprit* is higher or deeper or better than the other, but they rarely mix well, and there are good reasons for maintaining the distinction between their products.

To appreciate these reasons the reader must have a working notion of history as I and other practitioners conceive it. Without such a notion, conciliatory minds might find comfort in deciding that the difference I speak of is really one of semantics or at most of emphasis. Others, in less polite words, might suggest that from personal or professional pride I am upholding an unnecessary purism. "Perhaps he is right [would run the silent thought] about no new discipline being involved, but why not accept in historiography as big a dose of these new ingredients as anyone likes and call the result by some convenient name denoting the addendum's link with history?"

Such a compromise position is inadmissible. It negates the character, the virtues, and the uses of history, reducing it to a mere storehouse of data for the more rigorous, dedicated, and enlightening work of the analyst, typologist, and diagnostician of human behavior. What, then, are the criteria of history? There are four: Narrative, Chronology, Concreteness, and Memorability. History is first of all a story; "explanation" is incidental and some histories survive without it; for example, Barante's *History of the Dukes of Burgundy.* The story is, of course, intended as truth and it is therefore of a particular time, while also portraying time in motion—it is chronological in all senses; but the place may be narrow or wide. Next, in history concreteness prevails over other elements; the event, the what-happened is the main fare. This marks off history from analyses of situations, also actual, but not treated as stories; for example, institutional problems, cultural types, or the "anatomy" of comparable events such as revolutions. Concrete does not necessarily mean physical. There are "events" in the history of ideas, and ideas are themselves attached to common reality—to common motives and mundane accidents —through the originators, partisans, opponents, and would-be suppressors of ideas: all these passions and purposes can be organized into a narrative as dramatic as the rise and fall of states.[39]

Finally, history is the story of the memorable in the two meanings of

[39] See J. Barzun, "Cultural History: A Synthesis," in Fritz Stern, ed., *The Varieties of History* (New York, 1956), 387–402.

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"worth remembering" and "capable of being remembered." The first meaning points to selectivity, the second to intelligible pattern.

There can be no history without the gift of knowing what to leave out: no need to bring in gravitation to account for the Defenestration of Prague, of which it is nevertheless a fundamental factor. History easily survives the right kind of superficiality and does not depend on "depth" for its truth value. It is by its patterning, its composition in the strict sense, that understanding is conveyed and the master is made known. As Macaulay said of his own history, the arrangement is all-important, because it alone makes the welter intelligible and retainable. Composition is, on the large scale, what we are all so tender about on the small scale when we complain of our words being taken out of context.

Every one of these characteristic features of history calls for a competence above and beyond the ascertaining and verifying of facts by research. Fact is necessary but not sufficient to truth. Telling truth requires sagacity and style, or to put it less cryptically: esthetic judgment in composition and skill in the art of writing. It has been fashionable for a good while to affect derisive scorn for "literary history"—sometimes concealing more sensible criticisms under the charge. The phrase is in fact a tautology. Where is the history that is not a piece of writing? What is truly risible in a history is the pretense of rigor, science, demonstration—the panoply of the geometrical mind.

This issue of form versus method is for some reason frequently approached, then run away from. The editors of *History as Social Science*[40] remark that "the historian as social scientist knows that tables of statistics do not scan well. They break the rhythm of the text, and comments on numerical matters are often tedious. . . ." (That need not necessarily be so; the discomfort with numbers is something else, as I shall shortly suggest.) But the same writers also express surprise that historians appear "hostile to any kind of normalization of research procedure" and "resent the use of a word like re-tooling." There is, I am happy to say, no word *like* re-tooling, but the objection to it is not perverse. Like "normalization," tools suggest the laboratory, not to say the factory, where instrumental routine is proper and fruitful, though even in the lab inspiration is unscheduled.

Nor should it be inferred from its rejection of tools and procedures that history is not precise. It has its own precision, which like all products of the intuitive mind eludes definition. One might say that it is by its *rhetoric* that a history communicates the precise fact found by the researcher and which he wants the reader to see. And on the other side, the user of charts and tables should not deceive himself into thinking that these tools of his are not also a rhetoric: they impress, and as a craftsman

[40] David S. Landes and Charles Tilly, eds., *History as Social Science* (Englewood Cliffs, 1971), 12, 20.

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choosing and using them he is an impressionist.[41] Only, he demands, quite rightly, a different kind of perusal, which possibly yields a different kind of pleasure.

What necessitates in the composition of a history the unflagging exertion of the *esprit de finesse* is that without it chronology will come out muddled, concreteness blurred, narrative stymied, and memorability baffled. Great researchers have sometimes published as history compilations of findings that are even less readable than the driest chronicle, for they rebuff understanding whereas the chronicle only fatigues attention. But such works, though useful as quarries for later writers, do not create a precedent that suspends the requirement of adequate form.

Form is not a merit at everyone's command. The theorists I have been quoting exclaim with dismay: "It is almost as though some scholars felt that historians are born not made."[42] The remark shows the extent to which the intuitive character of historiography has been overlaid and forgotten. It is now assumed that Thucydides, Tacitus, Macaulay, and Henry Adams were "made" and can be reproduced in larger numbers with the normalization of methods—or else that they are not real historians, only great ones. Could it be that there lurks here an unconscious parallel with the growth of science, which required the production of large numbers of competent technicians, able to perform reliably on the small scale, singly or in teams, thus possibly reducing the value of the great scientist?

It will not do to think of such things; yet it may be that as long ago as 1904 Woodrow Wilson was already answering an unspoken argument to the same effect when he delivered his great judgment on Mommsen and his history:

> How would you critically distinguish it from a doctor's thesis? By its scope, of course; but its scope would be ridiculous if it were not for its insight, its power to reconceive forgotten states of society, to put antique conceptions into life and motion again, . . . and see a long national history singly and as a whole. Its masterly qualities it gets from the perceiving eye, the conceiving mind of its great author, his divination rather than his learning. The narrative impresses you as if written by one who has seen records no one ever deciphered. . . . Its insight is without rule and is exercised in singleness and independence. It is in its nature a thing individual and incommunicable.[43]

To DEFINE HISTORY by means of four qualities does not mean that it cannot accommodate every sort of data. Its relation to the whole human past

[41] Particularly in choosing his examples or units, the quantifier cannot help relying on his impression of what is similar and significant, for the record hardly ever provides ready-made instances of uniform shape, size, and weight, waiting to be singled out and totted up. Yet the rhetorical force of numbers strongly suggests this regularity and makes the most astute minds yield credence in a degree unrelated to validity. This phenomenon is commonplace in the evaluation of student credentials, where "scores" almost invariably prevail over verbal reports.

[42] Landes and Tilly, *History as Social Science*, 20.

[43] "The Variety and Unity of History," in Rogers, *Congress of Arts and Science*, 2: 15, 17.

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requires this flexibility, as does also its need to describe whatever is not ordinary—the terrain, the mood, the peculiar government or religion, or any other unique thing that plays a role in the story. One thinks at once of Macaulay's third chapter, "The State of England in 1685," which is a masterly social panorama.[44] In obedience to this same need, history at times splits itself into specialties, concentrating on the several ranges of human activity. Economic history is full of economic facts, diplomatic of diplomatic. A history of porcelain manufacture must talk about *pâte sur pâte* and a history of ideas about Occam's razor, neither of them a household article. They are or become familiar to those who take up these specialized histories, and the topical descriptions occupy so little space that nobody could claim a new science had resulted from their presence in the text.

The historian may in addition permit himself to digress in order to opine, argue, explain, speculate, moralize, and compare. The visions will differ and perhaps clash, but will be nonetheless desirable. As Pliny said, *Historia quoquo modo scripta bona est.* But these excursions must not become ends in themselves. The truly historical statements must greatly predominate over the rest. If "truly historical" needs illustration, here is one of the shortest: *Veni, vidi, vici* is a historical statement. "The mainspring of his character was conquest" is a psychological statement. "The net effect of his career was destruction, not creation" is a sociological statement. The Latin words may seem at first to lack the wished-for concreteness, but they are in fact a short short-story, concretized by the "I" and made chronological by the sequence of perfect tenses; after which, interpretative opinion is given in abstract, static words.

How radically unlike is the work done by students who *use* history for their purposes—to "find fresh answers" to questions social and typological—may be seen from a glance at the open page of their books, or at the daily paper.[45] What one may chance upon is a diagram in dots, crosses, and other marks, headed: "Computer-prepared map of violent incidents in France, 1840–1844," while on the opposite page is a geometrical outline of France, also crossed and dotted, showing the incidence of the incidents. Positive and negative numbers to three decimals express the absolute values applying to each of the levels of violence, side by side with a frequency distribution. A historian need feel no objection or distaste whatever at this use of history; rather, he rejoices that the ancient urge to record the past leads later on to such refined methods for dissecting it. But he is simultaneously

[44] *The History of England from the Accession of James II* (London, 1848–55). Note that Macaulay, never forgetting narrative, makes the survey of living conditions his third chapter, not his first as a modern writer intent upon setting and sociology might be tempted to do. Macaulay begins his story with the Romans in Britain and brings it quickly to the seventeenth century. He builds up momentum through a second chapter that ends with the death of Charles II and is then able to carry us safely over the necessarily static third.

[45] The quoted phrase is from a headline in the New York *Times,* July 3, 1971, over the report of findings in the new *Journal of Interdisciplinary History.* Diagrams and charts were also reproduced.

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conscious of one certitude and one doubt. He knows as he studies the charts in all directions that he is not reading history; and he feels an uneasiness about the capacity of the graphic-quantitative method for truth telling.

Both states of mind must be taken in conjunction with a declaration in *History as Social Science* that is close to the illustration provided by those charts:

> The transfer from other disciplines of insights often derived from the study of different subjects . . . entails obvious hazards. . . . Yet it is the kind of risk that the conscientious historian, thanks to his canons of evidence, is well equipped to take. This is not to say that these conceptual imports will make possible a history any more definitive than what has gone before. They open up new kinds and levels of understanding, and there will be other kinds and levels to follow.[46]

This is a distressing admission strengthening the one doubt. We borrow from statistics at the cost of great labor in producing and absorbing them and only get more of those vague "levels of understanding" that I discussed before in connection with psycho-history. What is worse, one is not sure of the "conscientious historian" and his handiwork in the findings. For numbers as a "geometry" throw into relief the force of the historian's intuitive "canons of evidence." To a nonquantifier it would seem injudicious to assimilate all political disturbances and count each as a measurable unit. Violence is not a homogeneous substance. Significance is not quantifiable. With his unfortunate temperament, the historian therefore wants to know the particularity of each incident and submit it to his skepticism—Was this one reported to the prefect as political when it was only a drunken brawl? Did the local authorities, for their own reasons, exaggerate the number? Isn't this report taken from a government subsidized newspaper? Did bureaucratic methods count some incidents twice? When we happen to know how unreliable crime statistics are, in every modern country and despite the clear definition of crimes, we may take more soberly the kind of understanding to be reached through counting.[47]

I am aware of the well-established convention with which such skepticism is greeted by the perennial "new thought": the historian is called "conservative"—even "reactionary"; he is accused of being descended on the distaff side from the dog in the manger. It is therefore important to say that on the contrary it is his living experience, not any adherence to old nostrums, that reinforces his conception of his art and his distrust of numbers. He knows at first hand, for example, that there is no proportion between the

[46] Landes and Tilly, *History as Social Science*, 45–47.

[47] See Sophia M. Robison, "A Critical View of the Uniform Crime Reports," *New York Statistician*, Oct. 1966, pp. 3–4. In any event, counting must not be mistaken for measurement in the scientific sense. To measure in that sense is possible only after a portion of a homogeneous material has been defined for use as a standard with which to express the quantity of any sample. It may be added that when this point is urged, social scientists sometimes retreat from their claim of quantification by pointing out that not all the natural sciences quantify, a rejoinder often coupled with the charge that the critic—a mere historian—does not know what science *really* is. (Conf., George C. Homans.)

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number of sales of a book and its influence. Toynbee's *Study of History* was a best seller without influence. Montesquieu and Marx were scarcely read in their original form and their thought changed the world. Rousseau was widely read and correspondingly powerful: you never can tell—until you look into it with your own eyes. A future archeologist who should find the Housing Bureau list of buildings in Greater New York would not know that the dominant form of architecture was the skyscraper. Numbers would tell him the opposite.[48]

There is no point in writing history if one is always striving to overcome and suppress its principal effect. That effect is to show over and over again the vagarious disorder of human affairs, the force of the irrational, the unstructured character of the past, and the futility of trying to make it say something unshakable in answer to system and method. True, there are reasons for human actions and a "logic of events" in their sequence. But multiplicity defeats regularity. History is not linear but miscellaneous; hence it does not isolate causes. It does not say: the Roman Empire declined *because* of Christianity, or *because* of malaria; the Commercial Revolution occurred *because* the herring shifted their breeding grounds in the North Sea. If the facts are true and relevant they are conditions, not causes. The more conditions we can enumerate and organize truthfully and intelligibly, the stronger will be our divination of past reality.[49] And that is what history is for: it is the counterpart of method, equally sound and peculiarly health-giving.

One would have thought that in our time of complaint against regimentation, when we are daily summoned to resist the overcrowding of the psyche with what is rigid and angular, the reading of history would have appeared as the antidote it is, instead of one more region to be colonized by mechanism. But apparently the obsessive drive to classify is stronger than the pain, and history has to repel the attacks of those who could be its first beneficiaries.

At the root of this conflict, brought on wholly by those who would "supersede" history, is their failure to see that the reality of the past differs from the reality of Nature. The physical world is a determinate order about which it is possible to make statements that are congruent, testable, and therefore permanent.[50] The past is an unstable construct of the imagina-

[48] To be sure, the numbers commonly used by historians about the growth of industry, population, or the cost of living are by and large trustworthy and certainly indispensable. But that familiar little table about the steady rise in pig-iron production every ten years is only an illustration. The historical truth lies in the statement of what happened and what followed, both in words.

[49] The acceptance of this complexity by social scientists is rare but perhaps increasing. Professor Jay W. Forrester's studies (e.g. *Urban Dynamics* [Cambridge, Mass., 1969]) are explicitly based on the principle of not excluding the unquantified; and the thesis of *Explanation in Archeology* by P. J. Watson, S. A. Leblanc, and C. L. Redman (New York, 1971) is that the authors' "explicitly scientific approach" does not displace or replace historical archeology: the two are "complementary, not conflicting" (p. 170).

[50] Despite the cant about its endless quest, science does establish definitive results. The new theory comes and upsets the old, but not by supplanting and destroying it; only by limiting its territory, enclosing it within a more comprehensive statement.

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tion. The sole force constraining the historical imagination is "the record," which is haphazard and also a product of the imagination. I mean, of course, the imagination of the real, seeking to find and to say without fraud what is or what was.

The result is that whereas there is one natural science, there are endless histories, overlapping and contradicting, argumentative and detached, biased and ambiguous. It is in these very ways that they truly render the past of mankind. With a minimum of merit any history is faithful, even when it contains errors since corrected. Here lies the best reason why histories and biographies should always be rewritten. Each viewer remakes a past in keeping with his powers of search and vision, whose defects readily disclose themselves in his work. Nobody is deceived. If it is objected that the multiplicity of accounts makes them all false, the answer is that a true one could be ascertained only by collation with the mind of God.

The expostulation, Of what use these flawed reports? is of course in order, because it could be argued that what I called the effect of history—its panorama of continuity in chaos—can be had just as fully from an astute synthesis of the daily paper. But the paper lacks solidity and charm (in the magic sense), it casts no spell. Its formative effect is nil; whereas the formative effect of history is great. Beyond the primitive pleasure in story, the exercise of the imagination and feelings, the satisfaction of curiosity, and the induced serenity of contemplation, there is in reading history a power that changes the mind and the person radically. Nobody can read Gibbon, Michelet, Burckhardt, Prescott, Froude, or Clarendon and be the same man afterwards. These panoramas of days lived do not simply confirm prejudices or substitute fresh ones. They rearrange, develop, and enlarge the total outlook of the soul upon the world. Each history produces a shock and a response akin to those produced by a great artistic experience. Reading history is indeed a species of artistic experience, combined with a cognitive, philosophic one—even though history does not confirm any one philosophy.

With strong emotion and fresh knowledge remaking the mind in this way, individual action also is changed. That fact is recorded in biography, where we learn of a particular impulse given by the study of history to careers in youth; or again, of the wise or stubborn policies of history-reading statesmen, all these influences helping to move the world. But more important still, history everywhere molds minds strong enough to stand without flinching the terror and confusion of existence. Such minds feel no temptation at the sight of seductive formulas, no urge to yield to the onslaught of all-too-lucid ideologies. They entertain no illusions about an unconditioned life, by which the unhistorical are often lured. In the daily encounter with ever-encircling parochialisms, the history-bred need wage no great struggle to be free. They turn back with impatience the flood of conventional knowledge, all plausibly wrong, that the powerful media keep spreading like a sterile sort of manure; and they are equipped to criticize and resist

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the advanced attitudes that misread the present from ignorance of the past.[51]

These means and powers are not uniform, of course; they are diverse and distributed, like history itself. Can nothing more sharply defined, more single-minded and precisely namable, then be attributed to history as its characteristic good? Though it produces no science, can it not at least adumbrate one great lesson, scheme, or set of laws? Certainly, high historical minds have sought laws and produced so-called philosophies of history. But it is not a quibble to say that in seeking a formula those minds were giving up their interest in history.[52] They were drawn on to something else, possibly something nobler. Yet the sad contortions and false simplicities of all philosophies of history so far written are enough to disqualify them at the base—no need to go further and ask for the promised "laws." What the reader culls gratefully here and there is the fine narrative passage, the sudden insight, the new concept, and its descriptive term. It is hard to imagine what we should do without *Zeitgeist,* "world-historical character," "Faustian culture," "ages of criticism and ages of creation," and many others that will readily occur. But students of history have not taken them at their face value. The words have first been naturalized into the common tongue, like the metaphors of psychoanalysis.

If it is asked what the prospects are for history today, the answer must be given in two parts, one very tentative. The surer prophecy is that if a great history is written, it will be recognized and read. The other part is description rather than prediction. History proper continues to nourish our spirit, though perhaps for a declining number.[53] And the historical sense itself tells us the reasons for this decline. Many—and perhaps the best—spirits are spurred by the visible decay of Western institutions to find "fresh answers" to what ails us; they use history, but do not cultivate it. They feel guilty when not at work on something of possible use immediately. Obviously the mode of science is the only one that will give them serviceable results.

[51] For example, lay reformers of the criminal law often speak as if in English law capital punishment for murder had always existed and been automatic, or again as if property rights had always had priority over the "values of life." They might think to better effect if they knew that "murder" originally meant a fine levied on a village where killing had been done; and that as late as the sixteenth century there could be at law no theft of diamonds, peacocks, or private papers, none of them being useful to husbandry: the Pentagon would have no case.

[52] ". . . ideological interpretations, Marxist or nationalist, conservative or liberal, religious or agnostic, providential or progressive, cyclical or linear, are a violation of his discipline and an offense to his knowledge." J. H. Plumb, *The Death of the Past* (Boston, 1970), 138.

[53] Observers note a drop in student enrollment for university studies in history, and revolutionary youth declares them useless, if not pernicious as the spectacle of an evil past. This feeling is understandable but fallacious; the antihistorian should not be a futurist, for how can he profit from the next ten years if he denies value to the last ten? These last necessarily brought him the light; he used them to evolve his plans and strengthen his purpose by means of experience. He cannot hope to leap from a disposable present and past to a pristine future shaped wholly by vision and will. Therefore the past which is accumulating every second behind him is as indispensable to his hope as hope itself. Only a cow, as Nietzsche showed, can achieve happiness by being a successful antihistorian.

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But there is a second emotional state that draws our contemporaries away from history. It is the resistance to organized discourse. All the arts, including literature, now attempt direct action on the sensorium. The novel, which for a century has thrived parasitically upon history, has rejected narrative: no one wants a story. In place of it many things are offered—notations, distillations of experience, enigmas, concentrated formulas—all again suggestive of the ubiquitous impulse toward science.[54] As for biography, except on the most popular level it suffers from the disrepute of the hero; it is difficult, even in a novel, to follow the falterings of an anti-hero for very long. The novel, finally, has abjured psychology, which is sensible enough when neither hero nor namable people are in the book. The novel is not an ultimate test, but if psychology is out, perhaps psycho-history is already one step behind the times.[55]

The mention of this possibility is not in me the sign of a Samson complex, attempting to bring down on the young and hopeful the fate that has temporarily overtaken Clio. I would close, rather, by suggesting that no one should prejudge the results of psycho-history and its siblings. Nothing I have said or implied about them is such a prejudgment or goes to the matter of the potential usefulness of their projected gains. That they are not new disciplines; that they are not history; that their sense of evidence is weak; that they usurp the name of science as well as of psychology; that human typology and social analogy, even when exact, are not sufficient guides to action; that technical jargon does not add to understanding; that the most innovative psycho-historian needs good histories to help him conceive, define, and direct his own work; that the theorists of the new genres should examine and concatenate their thoughts, rather than exhort and rally—these and a few other submissions I have tried to make clear enough for the profession to weigh and decide.

For my own part, I welcome the sober statistical study, the subtle social survey, the close analysis of character and motive, the comparative treatment of events and situations (I am a cofounder of a journal devoted to it), in a word the anatomy of anything colorably related to the living body of history.[56] And whenever such aids come to hand, I shall be happy to con-

[54] Natalie Sarraute won fame with her novel *Tropismes*, the title of which denotes the kind of animal reflex made by amoebas to light. When interviewed during the preparation of a later book she said: "In it I plunge deeper still into the kind of research that I haven't stopped pursuing since *Tropismes*." Quoted in Rayner Heppenstall, *The Fourfold Tradition* (New York, 1961), 270.

[55] It was a noteworthy coincidence that at the Conference, Professors Stern and Genovese mentioned their practice of assigning their students appropriate nineteenth-century novels as a means of making concrete the meanings of Marx, Nietzsche, and other social critics.

[56] I cite a few at random, for illustration: Staffan B. Linder, *The Harried Leisure Class* (New York, 1970) (economic); John Lukacs, *Historical Consciousness* (New York, 1968) (cultural); Isaiah Berlin, *Historical Inevitability* (London, 1954) (ethical); R. G. Collingwood, *The Idea of History* (London, 1946) (metaphysical); Theodore Caplow, *Two Against One: Coalitions in Triads* (Englewood Cliffs, 1968) (sociological); Pierre Grimal, *L'amour à Rome* (Paris, 1967) (institutional); Dora B. Weiner, "Le Droit de l'Homme à la Santé," *Clio Medica*, 5 (1970): 209–23 (medical-social); Amy B. Cruse, *The Victorians and Their Reading* (Boston, n.d.) (intel-

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clude with a paradox which I believe true and which should reconcile all parties: history, on one side, and analytic studies, across the way, are preliminary to each other.

lectual); F. H. Hayward, *Professionalism and Originality* (London, 1917) (educational-political); Alfred de Musset, *La Confession d'un Enfant du Siècle* (Paris, 1865), ch. 2 (autobiographical); and Freud, *Civilization and Its Discontents* (New York, 1930) (psychoanalytical).

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New Thoughts on German Nationalism

ROBERT M. BERDAHL

THIS ESSAY IS WRITTEN out of a sense of dissatisfaction with the manner in which historians have dealt with the problem of German nationalism. Its purpose is not to provide answers but to raise questions. It will attempt to point up some of the weaknesses of the conceptual framework that has long dominated the descriptions of German nationalism and to suggest some new avenues into which students of the problem might profitably turn.

ANYONE WHO EXAMINES the literature dealing with German nationalism in the nineteenth century will quickly be struck by the pervasive influence of Friedrich Meinecke's classic study, *Weltbürgertum und Nationalstaat,* which first appeared in 1907 and went through seven editions. To an uncommon degree Meinecke's analysis of the past was touched by the events of his own time; this accounts for his satisfaction at the culmination of German nationalism in the Bismarckian empire. It also accounts for the fact that the disaster of World War I soon forced him to revise some of his favorable judgments about German development. In the preface to the seventh edition (1927) he affirmed that he still held to the "high values" of "state, nation, and humanity"; whereas, however, he had earlier seen these values clearly manifested in nineteenth-century Germany, he saw them now "indistinctly, as in the mists." These limitations notwithstanding, the book has remained a seminal treatment of the growth of German national consciousness. Its continuing importance is confirmed by the fact that it has just recently appeared in an English translation.[1]

In *Weltbürgertum und Nationalstaat* Meinecke reflected the two domi-

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[1] Friedrich Meinecke, *Weltbürgertum und Nationalstaat* (Munich, 1908) (the book appeared at the end of 1907, but the publication date is always given as 1908). My citations are from the English translation, *Cosmopolitanism and the National State,* tr. Robert B. Kimber (Princeton, 1970), which is based on the seventh edition (1927).

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nant traditions of the German historical craft: historicism and idealism.[2] The historicist in him sought out those unique features of the German past. "Under close scrutiny," he wrote, "every nation proves to have its unique individual aspects. If the social sciences try to penetrate as deeply as possible into the typical and general characteristics of nations, the true historian will concentrate more on observing the particular features of an individual nation as faithfully and precisely as possible." German development, he tells us, was unique; it was different from that of France or most of the Western European nations because it sprang from a different source. West of the Rhine political unification preceded the development of national consciousness, the state preceded the nation. In France, for example, national consciousness derived from "the spirit of 1789, from the idea of the self-determination and sovereignty of the nation."[3] Germany, by contrast, had no common political experience; German national consciousness grew from its common cultural and ethnic heritage.[4] German national consciousness was shaped by the unique and changing features of German culture. "The state is not and does not become national through the will of the people or those who govern it but through the same means that language, customs, and faith are national and become national—through the quiet workings of the national spirit." Against the dictum of the Frenchman Ernest Renan—"The existence of a nation is a plebiscite of all the people every day"—Meinecke offered the German experience: "The principle is not: Whoever wants to be a nation is a nation. It is just the opposite: A nation simply is, whether the individuals of which it is composed want to belong to that nation or not. A nation is not based on self-determination but on pre-determination."[5]

Because culture formed the basis of German national consciousness, for Meinecke the active agent in the process of German unification was idea. It was a living idea, one that enabled, as he put it, "the captive limbs of the state and nation to be freed from their shackles."[6] The study of nationalism thus fell within the province of intellectual and political history. The idea of the national state was not born of the confluence of impersonal social and economic currents in the century, nor was it the result of the popular will of the "sluggish mass"; it grew from the thoughts of individual poets and writers. The idea of the nation was conceived in modern form in Germany at the end of the eighteenth century, implanted in the womb of cosmopolitanism. For Herder and Wilhelm von Humboldt, and

[2] For a critical discussion of these traditions and Meinecke's place in them, see Carlo Antoni, *From History to Sociology*, tr. Hayden V. White (Detroit, 1959), 86–118; W. Stark, introd. to his edition of Friedrich Meinecke, *Machiavellism: The Doctrine of Raison d'Etat and Its Place in Modern History*, tr. Douglas Scott (New York, 1965), xxi–xxvi.

[3] Meinecke, *Cosmopolitanism and the National State*, 10, 12.

[4] Meinecke did not define the nation in racial terms; however, when he wrote "A natural core based on blood relationship must be present in a nation," he acknowledged the importance of the ethnic definition of the nation. *Ibid.*, 9.

[5] *Ibid.*, 18, 205.

[6] *Ibid.*, 196.

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to a lesser extent for Fichte and Novalis, the nation was purely a cultural, not a political phenomenon. These men were nationalists because they were also cosmopolitans; for them the nation became the vehicle through which humanity realized its uniqueness and variety, through which it manifested the "true richness and range of human nature." Only gradually, Meinecke argued, did the idea of the nation separate itself from this universalism and "first become usable for the purposes of the state."[7] Political romantics such as Adam Müller and Karl Ludwig von Haller contributed to the growth of the idea of the nation state because they developed an appreciation for the individual state as a political institution, but they had little regard for the nation. Their followers—the Gerlachs and the band who surrounded Frederick William IV of Prussia—valued the traditional Germany, divided into separate states and linked through the Habsburg monarchy to the whole of Central Europe. According to Meinecke, this mixture of cosmopolitanism, humanitarian nationalism, and state power was given realistic form by the three great "liberators" of the German national idea: Hegel, Ranke, and Bismarck. The German national state in the nineteenth century developed from these diverse intellectual traditions; the Bismarckian empire was thus a healthy blend of Meinecke's ideals of state, nation, and humanity.

Meinecke's treatment of German nationalism, as I have suggested, has exercised a profound influence over the subsequent discussions of the problem. Historians who have rejected Meinecke's conclusions about the virtues of the Bismarckian empire and who are much further removed from the historicist and idealist schools of historiography have nevertheless accepted his thesis that German nationalism sprang from unique cultural roots, quite different from the political origins of nationalism in Western Europe. Hans Kohn, who devoted his life to the study of nationalism, distinguished between the "subjective" concept of nationalism—a common will and political self-determination, as expressed in the French Revolution—and the "objective" concept—blood, language, and culture as the common denominator of the German experience.[8] This is largely a restatement of Meinecke's categories. Kohn readily acknowledges the role of social and economic factors in the rise of nationalism, but as his recent book—*Prelude to Nation*

[7] *Ibid.*, 39, 44.

[8] "Nationalism in the West arose in an effort to build a nation in the political reality and the struggles of the present without too much sentimental regard for the past; nationalists in Central and Eastern Europe created often, out of the myths of the past and the dreams of the future, an ideal fatherland, closely linked with the past, devoid of any immediate connection with the present, and expected to become sometime a political reality. . . . Nationalism in the West was based upon a nationality which was the product of social and political factors; nationalism in Germany did not find its justification in a rational societal conception, it found it in the 'natural' fact of community, held together, not by the will of its members nor by any obligations of contract, but by traditional ties of kinship and status. German nationalism substituted for the legal and rational concept of 'citizenship' the infinitely vaguer concept of 'folk,' which, first discovered by the German humanists, was later fully developed by Herder and the German romanticists." Hans Kohn, *The Idea of Nationalism* (New York, 1944), 330–31; see also his *Prelude to Nation States: The French and German Experience, 1789–1815* (New York, 1967).

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States: The French and German Experience, 1789–1815—demonstrates, he considers the real actors in the national drama to have been the intellectuals: Möser, Klopstock, Herder, Müller, Kleist, Fichte, Schleiermacher, and others. Other historians—the German Hans Rothfels, the Frenchman Jacques Droz, and the Englishman Sir Lewis Namier, to name only a few—have described German nationalism in categories similar to those of Meinecke.[9] In short, historians of German nationalism have been largely preoccupied with the fact that it had origins different from those of Western Europe, that it sprang from German culture, and that it can be studied primarily in the ideas of its writers.

It would be wrong to suggest that these approaches to German nationalism have little to contribute to our understanding. Obviously the absence of a common political experience gave German nationalism a different hue; obviously the poets and thinkers were important in the formation of national consciousness. Nevertheless this cultural and ethnic emphasis in the study of German nationalism has certain weaknesses.

The first problem involved in emphasizing the cultural origins of German nationalism is the persistent dilemma of the historian of ideas: what was the relationship between the ideas and the political reality? How did the ideas of the intellectual elite become the experience of the nation? Or, if early nationalists merely articulated sentiments that were felt less keenly by a broad spectrum of the population, what new experiences generated the new nationalist thought? Finally, what happened between 1800 and 1848 that increased the appeal of nationalism as a political movement? This last question is never fully answered by those who have studied the growth of nationalism in the ideas of intellectuals. Meinecke, for example, relies on such vague generalizations as "the growth of realism." Ranke, he tells us, could never have shared the universalism of Humboldt or Fichte because "his consciousness and the consciousness of his time were too realistic and concrete for that."[10] Paradoxically he would make Ranke both the creator and the creation of the "consciousness of his time."

One thing upon which students of modern nationalism almost universally agree is that nationalism, at base, is rooted in the psychology of a people. The literature abounds in such descriptions of nationalism as "a condition

[9] Hans Rothfels, "Grundsätzliches zum Problem der Nationalität," *Historische Zeitschrift*, 174 (1952): 339–58; Jacques Droz, "Concept français et concept allemand de l'idée de nationalité," in *Europa und der Nationalismus, Bericht uber das III. internationale Historiker-treffen in Speyer—17. bis 20. Oktober 1949* (Baden-Baden, 1950), 111–13; Sir Lewis Namier, "Nationality and Liberty," in his *Vanished Supremacies* (New York, 1963), 31–53. See also Eugene N. Anderson, *Nationalism and the Cultural Crisis in Prussia, 1806–1815* (New York, 1939). In his preface Anderson comments: "The idea of nationalism, if one may employ the term 'idea' in Meinecke's sense of a living, molding cultural force, has exercised its own power over varied persons and conditions and put its stamp on them" (p. viii). See further, Otto Pflanze, *Bismarck and the Development of Germany: The Period of Unification, 1815–1871* (Princeton, 1963), 32–39. Pflanze also uses the categories of Meinecke, pointing out some of their limitations in "Nationalism in Europe, 1848–1871," *Review of Politics*, 28 (1966): 129–43.

[10] Meinecke, *Cosmopolitanism and the National State*, 216.

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of mind," a "state of mind," a "new form of consciousness." Carlton J. H. Hayes began his study by referring to nationalism as "the most significant emotional factor in public life today." Rupert Emerson has written, "The simplest statement that can be made about the nation is that it is a body of people who feel they are a nation." It seems highly questionable whether an intensified awareness of a common language and culture could alone provide the motive spring for German nationalism in the early nineteenth century. The attraction of nationalism as a political movement originated in the deeper psychological needs of some groups in the national community.[11]

The catalyst to which the development of German nationalism is always attributed is the Napoleonic occupation and the wars of liberation. It is true that these provided a crucial stimulus to national development. It is also true that they pushed nationalism in a number of different directions.[12] In addition to the few radical nationalists who began to demand political unity after 1815, the wars of liberation also produced the even more significant patriotism wherein loyalty was directed to the individual German states. Loyalty to the state as nation could be reconciled with the broader national cultural ideal, but it also acted as a counterweight to the demand for national political unity. Prussian conservative particularists, for example, denounced nationalism as a "swindle" as late as 1861.[13] Important as the wars of liberation were, they do not fully explain why the cultural idea of the nation was gradually transformed into the demand for closer political unity, nor why, once the external provocation of French occupation was removed, nationalism continued to capture the imagination of important groups in Germany.

A second limitation of the cultural or ethnic description of the origins of nationalism is that it bears little relationship to what nationalists finally demanded or achieved. In the debates at Frankfurt in 1848 Joseph Maria von Radowitz pointed out that the territorial boundaries of Germany were being endangered by "our conception of the national principle." The linguistic principle of nationality, he warned, had cost Germany a part of Posen; it had led the Czechs to seek autonomy in Bohemia, the Italians to demand a segment of the Tyrol, and it had put the future of Schleswig in doubt. In the end the delegates followed a double, contradictory answer to the problem: wherever the German tongue was spoken should be German; whatever

[11] See Leonard W. Doob, *Patriotism and Nationalism: Their Psychological Foundations* (New Haven, 1964), 4–9; Carlton J. H. Hayes, *Essays on Nationalism* (New York, 1926), 1; and Rupert Emerson, *From Empire to Nation: The Rise to Self-Assertion of Asian and African Peoples* (Cambridge, Mass., 1960), 102. One of the most significant discussions in this regard is David M. Potter, "The Historian's Use of Nationalism and Vice Versa," *AHR*, 67 (1961–62): 924–50. Potter argues that historians have only paid lip service to the "community of interest" as a source of nationalism. "The point is . . . that nationalism rests on two psychological bases instead of one: the feeling of common culture and the feeling of common interests" (p. 937).

[12] For the varieties of nationalism that developed, see Walter Simon, "Variations in Nationalism during the Great Reform Period in Prussia," *AHR*, 59 (1953–54): 305–21.

[13] This phrase was used to describe nationalism by the Prussian Conservatives in the program of their organization, the *Volksverein*, of September 1861. For a copy of the program, see Ludolf Parisius, *Deutschlands politische Parteien und das Ministerium Bismarcks* (Berlin, 1878), 42.

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territory was German should remain German. Some, like Wilhelm Jordan of Berlin, did not hesitate to state what considerations should be used in drawing boundaries. "Our right is the right of the stronger, the right of conquest," he said in justification of German control of Poland. At least one deputy recognized the inconsistency of the Assembly's attitudes toward nationality when he remarked, "We also engage in sophistry. Look at Posen, look at Schleswig—now one principle is on top, now the other. It goes around like a wheel."[14]

If cultural criteria became less important than state power in defining the nation in 1848, they continued to play a secondary role during the next two decades. The national state created by Bismarck excluded many Germans and included many non-Germans. As Otto Pflanze has argued, national unity for Bismarck was not an end in itself but a means for achieving a given objective: the expansion of Prussian power.[15] The Bismarckian state was not "pre-determined," it was "self-determined," not by popular sovereignty or the *Volk,* but by its leading statesmen. Much ink was spilled by nationalist publicists during the late summer of 1870 to prove that Germany should annex Alsace-Lorraine in order to restore to the provinces their greater German cultural heritage.[16] "Against their will we wish to give them back themselves," reasoned Treitschke. Bismarck was unmoved by such arguments except insofar as he hoped that common cultural bonds might ease the assimilation of the conquered provinces into the Empire and facilitate the acceptance of their annexation by the other European powers. Bismarck's primary consideration in annexing the territories was military security.[17] In the 1870s and the 1880s efforts to "Germanize" the Danes and the Poles also brought a new emphasis upon German culture. In all these instances, however, culture did not "form" the German national state, it legitimized it. As in 1848, the principle *cuius regio, eius natio* prevailed.

One final comment on the inadequacy of this approach to German nationalism. The stress on the uniqueness of the German national tradition has too easily shaped the normative judgments of German historians on both sides of the Atlantic. Originally most German historians found positive values in the unique development of their national past, in its departure from the Western European tradition. Meinecke's treatment of the German past, as we have noted, possessed a certain teleology, expressed, for example, when he used the notion of "pre-determination" to contrast the German development with the "self-determination" of France. The same may be said of Gerhard Ritter, who, in the new preface to the recent edition of his book, *The German Problem,* viewed the nation as a "historically bound and binding community"

[14] Wilhelm Jordan's outburst is reported in Sir Lewis Namier, *1848: The Revolution of the Intellectuals* (Garden City, 1964), 108–09. For the conflicting definitions of nationalism evident in the Frankfurt Assembly, see especially Pflanze, "Nationalism in Europe," 136–37.

[15] Pflanze, *Bismarck and the Development of Germany, passim.*

[16] Eberhard Kolb, "Bismarck und das Aufkommen der Annexionsforderung 1870," *Historische Zeitschrift,* 209 (1969): 318–56.

[17] Pflanze, *Bismarck and the Development of Germany,* 474.

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and lamented the fact that the younger generation of German intellectuals is turning away from it.[18] On the other hand, Western and some German historians, writing in the wake of the disasters that have befallen Germany in this century, have drawn opposite conclusions from their search of the German past. They have attributed the "German problem" in part to the type of nationalism that flourished there. Louis Snyder put it most bluntly: "Germany for a century and a half has been plagued by the wrong kind of nationalism. . . . Herein lies the tragedy of the German nation."[19] In short, a more balanced view might lay less stress on the uniqueness of the German national tradition.

Here, then, are a number of criticisms of the way historians have handled the subject of German nationalism. Too much attention has been given to the cultural and ethnic basis of German nationalism and thus to the contrast between Germany and Western Europe. This emphasis has been more useful in describing the "what" of nationalism than the "why"; it has described the ideas of individual nationalists, but it has not demonstrated why nationalism developed when it did or how it shaped the national state that actually emerged. Nationalism in Germany, as elsewhere, was defined not only by language or culture or ethnicity; nationality was also determined by the dictates of utility, by nationalists and politicians for whom nationalism was a functional concept.

WHILE IT SEEMS CLEAR that some new approaches to the study of German nationalism are required, it is less clear where we should turn to find them. Let me suggest some areas that we might explore more fully.

Some of the most stimulating literature on the subject of nationalism has recently been provided by those who have studied the emergence of the new nations in the non-European world.[20] Struggling to cut the tether of colonialism and to propel themselves into the modern world, these new states have embraced a revolutionary nationalism that, though obviously alien to the European experience in so many respects, may also contain some important similarities. For the newly emergent nations, as for Germany during the nineteenth century, nationalism is primarily a utilitarian concept. Studies of nationalism in these developing areas have yielded two generalizations that could profitably be applied to the origins

[18] Gerhard Ritter, *The German Problem* (Columbus, 1965), vii.

[19] Louis Snyder, *German Nationalism: The Tragedy of a People* (2d ed.; Port Washington, N.Y., 1969), 308.

[20] Two of the most suggestive studies are: The American Universities Field Service Staff, *Expectant Peoples: Nationalism and Development*, ed. K. H. Silvert (New York, 1963); and Rupert Emerson, *From Empire to Nation*. See also Emerson, "Nationalism and Political Development," *Journal of Politics*, 22 (1960): 3–28; Karl Deutsch, *Nationalism and Social Communication* (New York, 1953); Karl Deutsch, "Social Mobilization and Political Development," *American Political Science Review*, 55 (1961): 493–514; Samuel P. Huntington, *Political Order in Changing Societies* (New Haven, 1968); and S. N. Eisenstadt, *Modernization: Protest and Change* (Englewood Cliffs, 1966).

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of German nationalism. The first is that nationalism is generated among a people by the growing awareness of its economic backwardness and by the desire for a modern economy. One outstanding scholar of economic development has written that "the ideology of nationalism seems to be one of the requirements for the achievement of developmental goals in societies on a relatively low level of economic development."[21]

Alexander Gerschenkron has pointed out the relationship between economic backwardness and nationalism in Germany. The "New Deal in emotions" necessary for a "great and sudden industrialization effort" was in part derived, he argues, from the nationalism of Friedrich List.[22] List was one of the first to associate German economic retardation with the absence of national unity. He became acutely concerned about Germany's economic weakness during the depression that struck after 1816. Both German manufacturers, overcome by a flood of English goods after the termination of the Continental System, and German agriculture, plagued by crop failures and declining export markets caused in part by the English corn laws, suffered severely. In 1819 List and a number of businessmen founded the German Commercial and Industrial League to combat this crisis. During the next few years this group agitated for the creation of a German customs union, a plan that finally bore fruit with the completion of the *Zollverein* in 1834. These experiences, together with observations he made during his exile to the United States from 1825 to 1832, formed the background of List's economic doctrine elaborated most fully in *The National System of Political Economy*.[23]

The primary lines of List's argument are familiar. He began with an attack on what he called the cosmopolitan economic theory of Adam Smith, according to which the general prosperity—"the wealth of nations"—is the result of the division of labor and the free exchange of goods. Actually, List asserted, this would lead only to the prosperity of the advanced nations, not to that of the more backward ones. For List, Germany's natural antagonist was not her traditional enemy France, but England with its free trade, the objective of which was to keep Germany in an underdeveloped status, supplying the English world with "children's toys, wooden clocks, and philological writings." In contrast to Smith's doctrine List proposed the "national system of political economy," which

[21] Bert F. Hoselitz, "Nationalism, Economic Development, and Democracy," in Otto Feinstein, ed., *Two Worlds of Change* (Garden City, 1964), 250.

[22] Alexander Gerschenkron, "Economic Backwardness in Historical Perspective," in Bert F. Hoselitz, ed., *The Progress of Underdeveloped Countries* (Chicago, 1952), 24. For a recent critique of List's diagnosis of German economic backwardness, see Richard H. Tilly, "Los von England: Probleme des Nationalismus in der deutschen Wirtschaftsgeschichte," *Zeitschrift für die gesamte Staatswissenschaft*, 124 (1968): 179–96. Tilly also points out that concern over Germany's backwardness in relation to England first appeared around 1780. See also Hans Gehrig, *Friedrich List und Deutschlands politisch-ökonomische Einheit* (Leipzig, 1956).

[23] Friedrich List, *Das nationale System der politischen Ökonomie* (Stuttgart, 1841). All my citations are from the English edition, *The National System of Political Economy*, tr. Sampson S. Lloyd (London, 1904).

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"teaches how a given *nation* in the present state of the world and its own special relations can maintain and improve its economic conditions." The wealth of a nation depends upon its "productive power," the cultivation of all its resources, labor, capital, and intelligence. No nation can fully realize these productive powers unless it passes beyond the agricultural stage of development and achieves a balance between agriculture, industry, and commerce; for Germany to neglect industrial development would be like cutting off a "limb of its body." In these early stages of development the intervention of the state is essential to protect these productive powers. "The industrial history of nations," he wrote, "and none more clearly than that of England, proves . . . that a perfectly developed manufacturing industry, an important mercantile marine, and foreign trade on a really large scale, can only be attained by means of the interposition of the power of the state." The state power, however, cannot be brought to bear unless it is a unified state; "the unity of the nation becomes the fundamental condition of prosperity."[24]

In assessing the needs of Germany List declared that the *Zollverein* should be expanded to include Holland and Denmark. He wrote, "A natural consequence of this union must be the admission of both these countries into the German Bund, and consequently into the German nationality, whereby the latter will at once obtain what it is in need of, namely fisheries, naval power, maritime commerce, and colonies."[25] On other occasions he advocated the expansion of Germany eastward into the Danube basin. Hans Kohn has concluded from this that List was "one of the most extreme Pan-German imperialists."[26] Such a conclusion is unwarranted. List was an expansionist, to be sure, and his model was the American westward movement, but his use of the concept of the nation has little to do with Pan-German imperialism. His suggestion that admission of the Dutch and the Danes to the German Confederation would thereby admit them to the German nationality implies a concept of the nation that has little ethnic basis. The nation for List was a utilitarian concept; it was the vehicle through which economic advancement takes place. List's nation was not "pre-determined," it was avowedly "self-determined" in the same manner as Bismarck's, by need and common interest rather than culture.

List was not alone. The entire "older" historical school of economics, led by Wilhelm Roscher, Bruno Hildebrand, and Karl Knies, also rejected the cosmopolitanism of the classical school of economics. Although their theories differed in many details, as nationalists and historicists they sought an economic policy that would reconcile German historical development with the requirements of a modern economy.[27]

[24] *Ibid.*, 106, 99, 144, 132.
[25] *Ibid.*, 143.
[26] Kohn, *Idea of Nationalism*, 322.
[27] Feitel Lifschitz, *Die historische Schule der Wirtschaftswissenschaft* (Bern, 1914), 66–107,

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Karl Marx, operating from quite different assumptions, reached many of the same conclusions. The right of nationhood, he maintained, was not derived from common blood, or geography, or consciousness of common traditions and culture; the sole justification for national existence was the advancement of the economy. Accordingly, small nations, such as Denmark or Poland, had no right to statehood.[28] As early as 1845 Marx blamed Germany's preoccupation with idealist philosophy on economic backwardness. Germans, he wrote,

> feel called upon to sit in judgment on the whole world and see in Germany the fulfillment of all history. We have frequently noted that this inflated and extravagant national arrogance corresponds to an utterly petty, philistine, and backward existence. If national narrow-mindedness is everywhere repulsive, it becomes actually loathsome in Germany, for there it is coupled with the illusion that the Germans are above nationalism and practical interests, in contrast to those nations that have the frankness to admit their national narrow-mindedness and their dependence on practical interests.[29]

Marx was never a nationalist. But he advocated German unification in 1848, and when it became apparent that national unity could only be achieved by the military might of authoritarian Prussia, he supported that too. National unity would bring economic advancement, and with it, greater proletarian class consciousness.[30]

It is possible simply to pigeonhole the nationalism that sprang from these economic factors as "economic nationalism," the ideology of an aggressive capitalist elite in search of a wider market and greater state support, or, as in the case of Marx, an ideology that would serve the long-range interest of a proletarian revolution. As the work done on developing areas in the world today shows, however, it is necessary to probe even deeper. And this leads us to the second generalization that can be gleaned from the non-European experience. It is that nationalism is itself reinforced by economic development, that the process of economic modernization breaks down traditional society, thereby providing both the means and the psychological need for the creation of the consciousness of a broader community, the nation.[31]

121–98; Artur Sommer, "Ueber Inhalt, Rahmen, und Sinn Aelterer Stufentheorien," in Edgar Salin, ed., *Synopsis: Festgabe für Alfred Weber* (Heidelberg, n.d.), 535–65; Albert Müssigang, *Die soziale Frage in der historischen Schule der deutschen Nationalökonomie* (Tübingen, 1968), 81–117. Both Hildebrand and Knies were active in the nationalist cause. Hildebrand's political beliefs cost him his teaching post at Marburg in 1846; he served in the Frankfurt Assembly in 1848, then fled to Switzerland, where he remained until 1861. Knies, too, went into exile in Switzerland after 1848.

[28] Solomon Bloom, *The World of Nations: A Study of the National Implications in the Work of Karl Marx* (New York, 1941), 34–35, 44.

[29] Karl Marx, "Die Deutsche Ideologie," in *Historisch-kritische Gesamtausgabe, Erste Abteilung: Sämtliche Werke und Schriften mit Ausnahme des "Kapital,"* ed. D. Ryazanov (Moscow, 1927), 5: 445–46. I have used the translation from "The German Ideology" found in Bloom, *World of Nations*, 139.

[30] Bloom, *World of Nations*, 140–50; George Lichtheim, *Marxism: An Historical and Critical Study* (New York, 1961), 72, 82–83.

[31] On the basis of his study of nationalism in Asia and Africa, Rupert Emerson has concluded

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It is clear that even the early stages of economic development bring forth a set of circumstances that reinforce the quest for a national community. The process of economic modernization involves a radical transformation of the traditional structure of an agrarian society. It involves the destruction of an older social order in which the bonds of individual loyalty and dependency are based on personal and local obligations; it requires the creation of a new society in which individuals become highly dependent on many others from whom they are far removed personally and geographically. For men living on the land, loyalties extend outward in circles of diminishing intensity—to their village community or estate, to their district, and to their "country."[32] The result is a high level of social integration. This is the basis of Weber's description of a "status" society. It is integrated because the individual's own view of his status in society corresponds closely to the general conception held by society at large.[33] The development of a modern economy intrudes upon this set of relationships, first in the form of commercial agriculture, which alters or destroys traditional lord-peasant relations, later in the form of industrialism, which necessitates greater geographic and social mobility. The result is the destruction of traditional allegiances and a degree of social disintegration.[34] In the face of such disintegration nationalism acts as a force for the reintegration of society. As one student of this process in developing areas has noted, "Nationalism as a social value has been the major cohesive force to date within each separate modern society, and . . . its existence in underdeveloped areas is a necessary part of the process of development."[35]

that "nationalism is normally associated with deep-running social ferment and change which disrupt the older order of society and bring about a rise in social consequence and awareness of ever-widening segments and classes of people at large." "Nationalism and Political Development," 19–20.

[32] Namier, "Nationality and Liberty," 37. Namier points out that in France *pays* is still used for the various provinces and that in eighteenth-century England "country" was frequently used for "county."

[33] See especially Reinhard Bendix, *Nation-Building and Citizenship* (New York, 1964), 40–48.

[34] Wolfram Fischer uses three concepts to describe this social disintegration. The first he calls "decorporization," which refers to the breaking up of the corporate or *ständisch* structure of society. The second, "disproportionization," refers to the changing proportions between these corporate groups, i.e., the numerical growth of the lower strata. The third he defines as "demoralization," which results from "the loss of identity between the objective societal order and the subjective will of the individual." "Social Tensions at Early Stages of Industrialization," *Comparative Studies in Society and History*, 9 (1966): 76–77.

[35] Silvert, introd. to *Expectant Peoples*, 26. In another interesting study, *The Passing of Traditional Society: Modernizing the Middle East* (Glencoe, 1958), Daniel Lerner makes the following observation: "Empathy is . . . the inner mechanism which enables newly mobile persons to *operate efficiently* in a changing world. Empathy, to simplify the matter, is the capacity to see oneself in the other fellow's situation. This is an indispensable skill for people moving out of traditional settings. . . . High empathic capacity is the predominant personal style only in modern society, which is distinctively industrial, urban, literate, and *participant*. Traditional society is nonparticipant—it deploys people by kinship into communities isolated from each other and from a center . . . it develops few needs requiring economic interdependence; lacking the bonds of interdependence, people's horizons are limited by locale and their decisions involve only other *known* people in *known* situations. Hence there is no need for a transpersonal common doctrine formulated in terms of shared secondary symbols—a national 'ideology' which

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It may be useful in this regard to distinguish between the "idea" of the nation and the "ideology" of nationalism. The idea of nationality implies only that there is something unique about a people because of culture, language, race, or historical development; such a view may be seen, for example, in the writings of Herder. Nationalism as an ideology is functional. It serves a definite purpose of elites, as, for example, furthering economic development or binding a community together during a period of social upheaval. As an ideology it forms the "link between action and fundamental belief."[36] Indeed, German nationalism in the early nineteenth century may be more fully understood if we grasp the relationship between nationalism and socialism in developing nations. Both ideologies developed simultaneously in the process of modernization; by promising to liberate men from the oppression associated with that process both provide a moral basis for action. Reinhard Bendix has concluded:

> The desire of the workers for full citizenship and the search of the intellectuals for a power capable of removing the backwardness of their country, had a common pre-condition in the prior decline of kinship ties, religious belief, linguistic affiliation, territorial and racial communalism. . . . By these prior developments men were freed very gradually for such alternative solidarities as those of social class and national citizenship.[37]

What is especially lacking in German historiography is a study of the origins of German nationalism from this point of reference. Recent studies have given new emphasis to the economic and social bases of German unification. Helmut Böhme stresses those economic factors that accounted for the emergence of Prussian dominance and the exclusion of Austrian influence in Germany. Theodore Hamerow ties together the social, economic, and ideological impulses leading to German unification. Hans-Ulrich Wehler has pointed out how Bismarck attempted to utilize "social imperialism" as a means of integrating German society.[38] But there has been no attempt to study nationalism as a response to the needs of German modernization prior to 1848. It is possible here only to suggest some of the factors that need to be studied from this standpoint.

As in other developing areas, much of the tension present in Germany after 1815 can be traced to the gradual transformation of the traditional

enables persons unknown to each other to engage in political controversy or to achieve 'consensus' by comparing their opinions" (pp. 49–50).

[36] David Apter, *The Politics of Modernization* (Chicago, 1965), 314.

[37] Reinhard Bendix, "Concepts in Comparative Historical Analysis," in Stein Rokkan, ed., *Comparative Research across Cultures and Nations* (Paris, 1968), 78. David Apter maintains that "nationalism and socialism are ideologies that, better than most, provide the coherence necessary for retraditionalization during the process of change." *Politics of Modernization*, 340.

[38] Helmut Böhme, *Deutschlands Weg zur Grossmacht* (Cologne, 1966); Theodore Hamerow, *The Social Foundations of German Unification, 1858–1871* (Princeton, 1969); Hans-Ulrich Wehler, *Bismarck und der Imperialismus* (Cologne, 1969); see also Wehler's *Krisenherde des Kaiserreichs* (Göttingen, 1970), 113–61.

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social structure and the inability of the established political order to adjust to these changes. The result was a widening gulf between state and society.[39] In 1794, for example, Prussia issued the *Allgemeines Landrecht,* a general law code so comprehensive in nature that it has been considered the functional equivalent of a constitution.[40] The *Allgemeines Landrecht,* however, was based largely on an earlier notion of society; it divided Prussian society into separate Estates and painstakingly enumerated the rights and privileges of each. It represented the concerted effort of the Prussian bureaucracy to retain the essential elements of the traditional order at a time in which the older structure of society was losing its foundation. For a brief period of the reform era a more liberal bureaucracy attempted to make the state responsive to the demands of a changing society; after 1815 that effort failed and those classes that found little meaning or value in the traditional order became increasingly alienated. Similar experiences were repeated elsewhere in Germany. In Württemburg after 1815, when the middle-class liberals submitted their demands, the struggle between the monarch and the aristocracy became a three-cornered conflict over "the good old law."[41] The result, as one historian has described it, was "emotional turmoil at all levels of society, for the old orientations of loyalty, status, and law wrenched apart and twisted into new."[42]

The "emotional turmoil" evident in these social changes may also be seen in the important demographic shifts in Germany after 1815.[43] The massive increase in population (thirty-eight per cent between 1815 and 1845), the growth of cities, and the shift from the countryside to the city—these are problems that the historians of nationalism have generally ignored. Was the ideology of nationalism used by urban elites to command the loyalty and obedience of the new urban masses for whom the traditional loyalties of kinship and tradition no longer existed?

Overpopulation, economic hardship, and political unrest combined to form a steady stream of emigration from Germany. This emigration itself

[39] On this theme, see Reinhart Koselleck, "Staat und Gesellschaft in Preussen," and Werner Conze, "Das Spannungsfeld von Staat und Gesellschaft im Vormärz," in Conze, ed., *Staat und Gesellschaft im deutschen Vormärz, 1815–1848* (Stuttgart, 1962), 79–112, 207–69. For a larger study of Prussia, beginning with the *Allgemeines Landrecht* and ending with the revolution of 1848, see Reinhart Koselleck, *Preussen zwischen Reform und Revolution* (Stuttgart, 1967).

[40] See Koselleck, "Staat und Gesellschaft in Preussen," 80.

[41] On the struggle over the "good old law," see Leonard Krieger, *The German Idea of Freedom* (Boston, 1957), 237–41.

[42] Mack Walker, *Germany and the Emigration, 1816–1885* (Cambridge, Mass., 1964), 2.

[43] Between 1815 and 1845 the population of Germany grew from 25 to 34.25 million. Most of this gain was in the cities. Berlin grew from 191,500 in 1815 to 403,586 in 1847; during the same three decades other cities grew comparably, in many cases doubling or tripling their size. Equally important, much of this growth was due to the influx of people from the countryside. Between 1837 and 1844, for example, the population of Berlin increased a total of 101,130, only 15,453 of which could be attributed to an excess of births over deaths. See Theodore Hamerow, *Restoration, Revolution, Reaction* (Princeton, 1958), 19–20; Hajo Holborn, *A History of Modern Germany* (New York, 1969), 3: 7–8. For an annual rate of population growth, broken down by decades, see D. V. Glass and E. Grebenik, "World Population, 1800–1950," in *The Cambridge Economic History of Europe,* 6, pt. 1, ed. H. J. Habakkuk and M. M. Postan (Cambridge, 1965): 62.

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served to spur nationalism. The increasing number of Germans emigrating to America after 1840 served to point up the backwardness and political impotence of a divided Germany. Political division had left Germany behind in the establishment of colonies; now those who left strengthened foreign states, not Germany itself. One representative in a South German diet lamented in the 1840s: "Every state seeks solid ground beyond the seas; even little Belgium founds colonies. Only Germany, with its forty millions, remains idle; it leaves to pure chance one of the most important of national concerns, the fate of many thousands, and the honor of its name in the New World."[44] Friedrich List argued that Germany must expand eastward, providing settlements for Germans who would otherwise be lost to the Fatherland forever. But that necessitated a strong national state, which Germany lacked.[45]

It is significant that those classes in the society that were the most closely tied to the traditional social structure—the aristocracy and the peasantry—were the least prone to nationalist sentiment.[46] Prussian conservative aristocrats, though supporters of a *kleindeutsch* economic union, professed hostility to nationalism, and their Conservative party did not embrace it fully until 1876. After 1848, nevertheless, the ruling elite found it increasingly necessary to use nationalism to buttress its position.[47]

On the other hand, those groups that had broken loose from the traditional order, who felt alienated from it, often sought to find an identity within the national movement. Such was the case with the youth, for example, in the *Burschenschaften*.[48] It was also the case with Jewish intellectuals such as Heinrich Heine and Ludwig Börne, who could not gain access to traditional society. Börne may have reflected the patriotism of many Jews when he wrote,

Because I was born without a fatherland, my desire for a fatherland is more passionate than yours, and because my birthplace was not bigger than the *Juden-*

[44] Quoted in Walker, *Germany and the Emigration*, 124.

[45] See List's essay, "Die Ackerverfassung, die Zwergwirtschaft, und die Auswanderung" (1842), in Ludwig Hausser, ed., *Gesammelte Schriften* (Stuttgart, 1850), 2: 150–234; Walker, *Germany and the Emigration*, 114–18.

[46] For the posture of the various social classes on the issue of national unification, see Hamerow, *Social Foundations of German Unification*, 380–99.

[47] Karl Deutsch maintains that it is common for the traditional upper classes, especially aristocracies, to assume the leadership of a national movement, even against their will, in order to sustain political pre-eminence. *Nationalism and Social Communication*, 75–76.

[48] Youth, by definition, stands between a traditional order and a newly forming society. Erik H. Erikson has pointed out why ideologies are especially appealing to the youth. "Youth stands between the past and the future, both in individual life and society. It also stands between alternate ways of life. . . . Ideologies offer to the members of this age group overly simplified and yet determined answers to exactly those vague inner states and those urgent questions which arise in consequence of identity conflict. Ideologies serve to channel youth's forceful earnestness and sincere asceticism as well as its search for excitement and its eager indignation toward that social frontier where the struggle between conservatism and radicalism is most alive. On that frontier, fanatic ideologists do their busy work and psychopathic leaders their dirty work; but there, also, true leaders create significant solidarities." *Young Man Luther: A Study in Psychoanalysis and History* (London, 1958), 42. See also David E. Apter, ed., introd. to *Ideology and Discontent* (New York, 1964), 16–46.

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gasse and everything behind the locked gates was a foreign country to me, therefore for me now the fatherland is more than the city, more than a territory, more than a province. For me only the very great fatherland as far as its language extends, is enough.[49]

Areas of Germany, such as Baden or the Rhineland, where traditional society had broken down, either through the reorganization of Napoleon or the strains of population pressure and class tension, were also the most nationalist. The *Mittelstand*, defined only negatively in the Prussian *Allgemeines Landrecht* as composed of everyone who was neither peasant nor noble, was the most susceptible to the appeal of nationalism. The working-class movement developed ties with the nationalist cause very early in the century.[50] Nationalism frequently was the means by which the middle class expressed its frustration with the conservative order in Germany. In trying to launch railroad construction, for example, middle-class entrepreneurs often found themselves pitted against those old elites that had a vested interest in German particularism. Hampered by reactionary bureaucracies when they attempted to extend rail lines across territorial frontiers, entrepreneurs saw national unification as the only solution.[51] Moreover, liberal nationalists viewed the railroad as a major ally. Jakob Venedey commented in 1835:

In a completely different way and a much more perceptible fashion than is so often boasted of the *Zollverein*, the railroads will bring down all the internal borders in Germany. Ten years after all the large and capital cities are linked together by the railroad, Germany will be a completely different country, and the prejudices which have so splintered the German people up to now, which have so facilitated the rule of its oppressors, will have ceased to exist.[52]

A NEW PERSPECTIVE on the origins of German nationalism is needed. We must explore the relationship to the national movement of those individuals and groups that were especially concerned that Germany overcome its economic backwardness. More ought to be done to measure the impact of internal migration on national consciousness. We should discover the dimensions of the tensions of early industrialization at the local level and their effect on nationalist sentiment. However sketchy and tenuous the

[49] Ludwig Börne, *Gesammelte Schriften* (Milwaukee, 1858), 5: 31–32. I have used the translation as it appears in Koppel S. Pinson, *Modern Germany* (New York, 1966), 67–68. The relationship of German Jews to the nationalist movement would provide an excellent case study for the psychological roots of nationalism. For a description of Jews seeking assimilation, see Toni Oelsner, "Three Jewish Families in Modern Germany: A Study in the Process of Emancipation," *Jewish Social Studies*, 4 (1942): 241–68, 349–98.

[50] Werner Conze and Dieter Groh, *Die Arbeiterbewegung in der Nationalen Bewegung* (Stuttgart, 1966), 13–40.

[51] Dietrich Eichholtz, *Junker und Bourgeoisie vor 1848 in der preussischen Eisenbahngeschichte* (East Berlin, 1962), 1–12, 33–36.

[52] Quoted in *ibid.*, 7–8.

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hypotheses presented here about the origins of German nationalism may be, they offer possibilities for research that ought to be pursued further. In the future historians of German nationalism must concern themselves with more than its ethnic and cultural roots. While these factors are important, they must be seen less as the primary causes for the demand for national unity than as ones used to legitimize that demand. Nationalism in Germany, as in all societies, served both as an instrument for those who wanted to overcome economic backwardness and as a means of assuring social cohesion during the passage from traditional to modern society.

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The Founding Fathers and Slavery

WILLIAM W. FREEHLING

ONLY A FEW YEARS AGO, in a historical age now grown as arcadian as Thomas Jefferson himself, no man needed to defend the Founding Fathers on slavery. However serious were their sins and however greedy seemed their pursuits, the men who made the American Revolution were deemed to have placed black slavery at bay. Patriots such as George Washington, historians used to point out, freed their slaves. If Jefferson emancipated few of his, the condemnation of Jeffersonian ideology and the curse of a declining economy were fast driving Virginia's slavery to smash. Only the fabulous profits made possible by Whitney's invention of the cotton gin and the reactionary abstractions perpetuated by Calhoun's repudiation of Jefferson breathed life into the system and waylaid the Fathers' thrust toward peaceful abolition.

This happy tale, once so important and so widely believed, now lies withered by a decade of attack. Scholars such as Robert McColley, Staughton Lynd, William Cohen, and Winthrop Jordan have assaulted every aspect of the old interpretation.[1] Some revisionists write to correct excesses in the former view. Others are driven by a New Leftist contempt for reformers who repudiate radicalism and a modern-day repugnance for liberals contaminated by racism. Whatever their separate reasons and however qualified their individual positions, these scholars, taken together, have hammered out a new image of the Founding Fathers. The image is not attractive. In an era of racial turmoil the racist taints portrayed by Jordan seem even more grotesque than the grasping materialism described by Beard.

The Declaration of Independence, it is now argued, was a white man's

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[1] Robert McColley, *Slavery and Jeffersonian Virginia* (Urbana, 1964); Staughton Lynd, *Class Conflict, Slavery and the United States Constitution* (Indianapolis, 1968); William Cohen, "Thomas Jefferson and the Problem of Slavery," *Journal of American History*, 56 (1969): 503–26; Winthrop D. Jordan, *White Over Black: American Attitudes Towards the Negro, 1550–1812* (Williamsburg, 1968). For the fullest summation of the position, see Donald L. Robinson, *Slavery in the Structure of American Politics* (New York, 1971).

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document that its author rarely applied to his or to any slaves. The Constitution created aristocratic privilege while consolidating black bondage. Virginia shrank from abolition, for slave prices were too high and race fears too great. Jefferson himself suspected blacks were innately inferior. He bought and sold slaves; he advertised for fugitives; he ordered lashes well laid on. He lived in the grand manner, burying prayers for freedom under an avalanche of debt. In all these evasions and missed opportunities Jefferson spoke for his age. For whatever the virtues of the Founding Fathers, concludes the new view, they hardly put slavery on the road to ultimate extinction. It seems fitting, then, that when Southerners turned their backs on the Declaration and swung toward reaction in the wake of the Missouri crisis, the sage of Monticello himself helped point the way.

Many admirers of Jefferson, aware of a brighter side, scorn this judgment and yearn for a reassessment. The following essay, while in sympathy with their position, is not written for their reasons. More is at stake than Thomas Jefferson; indeed Jefferson's agonized positions on slavery are chiefly important as the supreme embodiment of a generation's travail. Moreover, the historian's task is not to judge but to explain; and the trouble with the new condemnatory view is not so much that it is a one-sided judgment of the Founding Fathers as that it distorts the process by which American slavery was abolished. The new charge that the Founding Fathers did next to nothing about bondage is as misleading as the older notion that they almost did everything. The abolitionist process proceeded slowly but inexorably from 1776 to 1860: slowly in part because of what Jefferson and his contemporaries did not do, inexorably in part because of what they did. The impact of the Founding Fathers on slavery, like the extent to which the American Revolution was revolutionary, must be seen in the long run not in terms of what changed in the late eighteenth century but in terms of how the Revolutionary experience changed the whole of American antebellum history. Any such view must place Thomas Jefferson and his contemporaries, for all their ironies and missed opportunities, back into the creeping American antislavery process.

IF MEN WERE EVALUATED in terms of dreams rather than deeds everyone would concede the antislavery credentials of the Founding Fathers. No American Revolutionary could square the principles of the Declaration with the perpetuation of human bondage. Only a few men of 1776 considered the evil of slavery permanently necessary. None dared proclaim the evil a good. Most looked forward to the day when the curse could be forever erased from the land. "The love of justice and the love of country," Jefferson wrote Edward Coles in 1814, "plead equally the cause of these people, and it is a moral reproach to us that they should have pleaded it so long in vain."[2]

[2] Thomas Jefferson to Edward Coles, Aug. 25, 1814, in Paul Leicester Ford, ed., *The Works of Thomas Jefferson* (New York, 1904–05), 11: 416.

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If the Founding Fathers unquestionably dreamed of universal American freedom, their ideological posture was weighed down equally unquestionably with conceptions of priorities, profits, and prejudices that would long make the dream utopian. The master passion of the age was not with extending liberty to blacks but with erecting republics for whites. Creative energies poured into designing a political City on the Hill; and the blueprints for utopia came to be the federal Constitution and American union. When the slavery issue threatened the Philadelphia Constitutional Convention the Deep South's ultimatums were quickly met. When the Missouri crisis threatened the Union Jefferson and fellow spirits beat a retreat. This pattern of valuing the Union more than abolition—of marrying the meaning of America to the continuation of a particular government—would persist, producing endless compromises and finally inspiring Lincoln's war.

The realization of the Founding Fathers' antislavery dream was blocked also by the concern for property rights articulated in their Declaration. Jefferson's document at once denounced slave chains as immoral and sanctioned slave property as legitimate. It made the slave's right to freedom no more "natural" than the master's right to property. Liberty for blacks became irrevocably tied to compensation for whites; and if some proposed paying masters for slaves, no one conceived of compensating South Carolina planters for the fabulous swamp estates emancipation would wreck.

The financial cost of abolition, heavy enough by itself, was made too staggering to bear by the Founding Fathers' racism, an ideological hindrance to antislavery no less important than their sense of priorities and their commitment to property. Here again Jefferson typified the age. As Winthrop Jordan has shown, Jefferson suspected that blacks had greater sexual appetites and lower intellectual faculties than did whites. This racism was never as hidebound as its twentieth-century varieties. Jefferson kept an open mind on the subject and always described innate differences as but his suspicion. Still it is significant, as Merrill Peterson points out, that Jefferson suspected blacks were inferior rather than suspecting blacks were equal.[3] These suspicions, together with Jefferson's painfully accurate prophecy that free blacks and free whites could not live harmoniously in America for centuries, made him and others tie American emancipation to African colonization. The alternative appeared to be race riot and sexual chaos. The consequence, heaping the cost of colonization on the cost of abolition, made the hurdles to emancipation seem unsurmountable.

Jefferson and the men of the Revolution, however, continually dreamed of leaping ahead when the time was ripe. In 1814, while lamenting his own failure, Jefferson urged others to take up the crusade. "I had always hoped," he wrote Edward Coles, "that the younger generation receiving their early impressions after the flame of liberty had been kindled in every breast . . . would have sympathized with oppression wherever found, and proved their

[3] Jordan, *White Over Black*, 429–81; Merrill D. Peterson, *Thomas Jefferson and the New Nation: A Biography* (New York, 1970), 263.

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love of liberty beyond their own share of it." As late as 1824, five years after his retreat in the Missouri crisis, Jefferson suggested a federally financed postnati abolition scheme that would have ended slavery faster than the plan proposed by his grandson, Thomas Jefferson Randolph, in the famed Virginia slavery debate of 1832.[4]

The ideological stance of Jefferson and other Founding Fathers on slavery, then, was profoundly ambivalent. On the one hand they were restrained by their overriding interest in creating the Union, by their concern for property rights, and by their visions of race war and miscegenation; on the other hand they embraced a revolutionary ideology that made emancipation inescapable. The question is, How was this theoretical ambivalence resolved in practical action?

The answer, not surprisingly, is also ambivalent. Whenever dangers to Union, property, or racial order seemed to them acute the Founding Fathers did little. In the short run, especially in those Deep Southern states where the going was stickiest, they did almost nothing. But whenever abolition dangers seemed to them manageable Jefferson and his contemporaries moved effectively, circumscribing and crippling the institution and thereby gutting its long-range capacity to endure.

The revisionist view of the Founding Fathers is at its best in emphasizing slavery's short-run strength in Jefferson's South. In Virginia both secure slave prices and frenzied race fears made emancipation a distant goal. Jefferson as legislator did no more than draft abolitionist resolutions, and his revisions of the Virginia slave code did little to ease the lot of slaves and something to intensify the plight of free blacks. Jefferson's proposed clause, requiring a white woman who had a black child to leave the state within a year or be placed "out of the protection of the laws," speaks volumes on why abolition came hard in Virginia. South of Virginia, where percentages of slaves and profits from staple crops ran higher, abolition was more remote. Planters who worked huge gangs of slaves in pestilential Georgia and South Carolina's lowlands never proposed peacefully accepting the end of their world.

The federal Constitution of 1787 also reflected slavery's short-run strength. Garrison's instinct to consign that document to the flames was exactly right, for the Constitution perpetually protected an institution the Fathers liked to call temporary. Safeguards included the three-fifths clause, destined to help make the minority South political masters of the nation for years, and the fugitive slave clause, destined to help return to thralldom men who had risked everything for freedom. Moreover, to lure Georgia and South Carolina into the Union, the Fathers agreed to allow any state to reopen the African slave trade for twenty years. When South Carolina seized the option from 1803 to 1807 the forty thousand imported blacks and their hundreds

[4] Jefferson to Coles, Aug. 25, 1814, Jefferson to Jared Sparks, Feb. 4, 1824, in Ford, *Works of Jefferson*, 11: 416, 12: 335–36.

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of thousands of slave descendants paid an awesome price for the creation of the white man's republic.

After the Constitution was ratified slavery again showed its strength by expanding over the West. "The years of slavery's supposed decline," Robert McColley points out, "were in fact the years of its greatest expansion."[5] In the age of Jefferson black bondage spread across Kentucky and engulfed Alabama and Mississippi. Furthermore, Jefferson as president acquired slave Louisiana, and Jefferson as elder statesman gave his blessings to the resulting diffusion of the system. If in the 1780s Jefferson had believed, as he did in 1819, that diffusing slavery made it more humane, the antislavery clause in the Northwest Ordinance might have been scotched and this essay could not have been written.

Slavery showed its strength not only in Jefferson's Virginia legislature, Philadelphia's Constitutional Convention, and Louisiana's black deltas but also at Monticello itself. By freeing their slaves George Washington and John Randolph lived up to Revolutionary ideals. These men, however, were exceptions. Thomas Jefferson, who freed nine while blithely piling up debts that precluded freeing the rest, was the rule. The plantation life style, with its elegant manner and extravagant tastes, lessened the chance of reducing debts and allowing quick manumission on a massive scale. That life style, in Virginia and throughout the South, was as integral a part of slavery as was South Carolina's hunger for Africans and the Southwest's commitment to cotton.

The master of Monticello, finally, revealed the towering practical strength of slavery in the notorious case of Sally Hemings, his mulatto house servant. Those who enjoy guessing whether Jefferson sired Sally's many offspring can safely be left to their own speculations. The evidence is wildly circumstantial and the issue of dubious importance. Of greater significance is the way Jefferson and his contemporaries handled the ugly controversy. Alexander Hamilton could cheerfully confess to illicit relations with a white woman and continue with his career. Jefferson's supporters had to ward off all talk of the embarrassing Sally, for interracial sex would ruin anyone's reputation. Nor could Jefferson handle the problem resolutely in the privacy of his own mansion. Firm action would, as Dumas Malone points out, "have looked like a confession that something was wrong on the mountain."[6] Better to look the other way as Sally's light-skinned children multiplied. Better to keep blacks enchained for a time than risk a nation polluted by allegedly lascivious Sallys. Better, in short, to live uneasily in a corrupted City on the Hill than blurt out the full horror of America's nightmare.[7]

The old view, then, that slavery was dying in Jefferson's South cannot

[5] McColley, *Slavery in Jeffersonian Virginia*, 3.

[6] Dumas Malone, *Jefferson the President: First Term, 1801–1805* (Boston, 1970), 498.

[7] See the stimulating comments on the matter in Jordan, *White Over Black*, 468, and Eric McKitrick, "The View from Jefferson's Camp," *New York Review of Books*, Dec. 17, 1970, p. 37.

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withstand the revisionist onslaught. The system was strong and, in places, growing stronger; and the combination of economic interest, concern for the Union, life style, and race prejudice made emancipationists rare in Virginia and almost nonexistent in South Carolina. Jefferson, no immediate emancipationist, refused as president to endorse an antislavery poem that had been sent to him for his approval. He could not, he said, "interpose with decisive effect" to produce emancipation. To interpose at all was to toss away other reforms.[8] Here as always Jefferson reveals himself as the pragmatic statesman, practicing government as the art of the possible. An idealist might fault him for refusing to commit political suicide by practicing utopian politics. But all the evidence of Robert McColley shows that as a practical politician Jefferson accurately gauged impassable obstacles. The point is crucial: long before Garrison, when Jefferson ruled, peaceful abolition was not possible.

What could be done—what Jefferson and his contemporaries did—was to attack slavery where it was weakest, thereby driving the institution south and vitiating its capacity to survive. In a variety of ways the Founding Fathers took positive steps that demonstrated their antislavery instincts and that, taken together, drastically reduced the slavocracy's potential area, population, and capacity to endure.

The first key reform took place in the North. When the American Revolution began slavery was a national institution, thriving both north and south of the Mason-Dixon line. Slaves comprised 14 per cent of the New York population, with other figures ranging from 8 per cent in New Jersey to 6 per cent in Rhode Island and 3 per cent in Connecticut and Pennsylvania. In these states, unlike Virginia, percentages of slaves were low enough to permit an unconvulsive variety of reform.

Still, prior to 1776, abolitionists such as John Woolman found the North barren soil for antislavery ideas. As John Jay recalled, "the great majority" of Northerners accepted slavery as a matter of course, and "very few among them even doubted the propriety and rectitude of it."[9] The movement of 1776 changed all this. The humanitarian zeal of the Revolutionary era, together with nonslaveholder hatred of slave competition and universal acknowledgment that the economy did not need slavery, doomed Northern slavery to extinction. In some states the doom was long delayed as Northern slaveholders fought to keep their bondsmen. Slavery was not altogether ended in New York until 1827 and in New Jersey until well into the 1840s. By 1830, however, less than one per cent of the 125,000 Northern blacks were slaves. Bondage had been made a *peculiar* institution, retained alone in the Southern states.[10]

[8] Jefferson to George Logan, May 11, 1805, in Ford, *Works of Jefferson*, 10: 141–42.

[9] Jay to Granville Sharp [1788], in Henry P. Jackson, ed., *The Correspondence and Public Papers of John Jay* (New York, 1890–93), 3: 342.

[10] Arthur Zilversmit, *The First Emancipation: The Abolition of Slavery in the North* (Chicago, 1967).

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No less important than abolition in old Northern states was the long and bitter fight to keep bondage from expanding. In 1784 Jefferson drafted a congressional ordinance declaring slavery illegal in all Western territories after 1800. The proposed law, keeping bondage out of Alabama and Mississippi no less than Illinois and Indiana, lost by a single vote, that of a New Jerseyite ill in his dwelling. Seldom has a lone legislator lost so good a chance to turn around the history of a nation. "The fate of millions unborn," Jefferson later cried, was "hanging on the tongue of one man, and heaven was silent in that awful moment."[11]

Three years later, in the famed Northwest Ordinance of 1787, Congress decreed slavery illegal immediately in the upper Western territories. The new law left bondage free to invade the Southwest. But without the Northwest Ordinance slavery might have crept into Illinois and Indiana as well, for even with it bondage found much support in the Midwest.

In the years before 1809 Indiana settlers, led by William Henry Harrison and the so-called Virginia aristocrats, petitioned Congress again and again to allow Midwestern slavery. Indiana's pro-Harrison and anti-Harrison parties were both proslavery; they disagreed only on the tactical question of how to force Congress to budge. When Congress refused to repeal the ordinance, the Indiana legislature in 1805 passed a black indentured servitude act, in effect legalizing slavery. Indiana census takers, more honest than the legislature, counted 237 slaves in the territory in 1810 and 190 in 1820.

In 1809, when the part of Indiana that was most in favor of slavery split off as the new territory of Illinois, the battleground but not the issue shifted. The climax to the territorial phase of the Midwestern quest for slavery came in the Illinois Constitutional Convention of 1818, when proslavery forces, after winning a bitterly contested election to the convention, settled for a renewal of the territorial indentured servitude law because they feared that an explicit slavery law might jeopardize statehood.

With statehood secured the battle over slavery in Illinois continued in the 1820s. The hero of the antislavery forces was Edward Coles, an enlightened Virginian deeply influenced by Madison and Jefferson. Coles, who came to Illinois to free his slaves and stayed to protect the Northwest Ordinance, narrowly defeated his proslavery rival for governor in 1822. In 1824 he helped secure, by the close vote of 6,640–4,973, final victory in a referendum on a proslavery constitutional convention. With Coles's triumph slavery had again been restricted to the South.[12]

The crusade for slavery in Illinois and Indiana, lasting over a quarter of a century and so often coming so close to victory, forms a dramatic example of the institution's expansive potential in the age of the Founding Fathers. The

[11] Quoted in Peterson, *Jefferson*, 283.

[12] The Indiana-Illinois story can best be followed in Jacob P. Dunn, Jr., *Indiana: A Redemption from Slavery* (Boston, 1888); Theodore Calvin Pease, *The Story of Illinois* (Chicago, 1949), 72–78; and Adrienne Koch, *Madison's "Advice to My Country"* (Princeton, 1966), 144–51.

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proslavery drive was turned back in part because of race phobias and economic desires that obsessed nonslaveholding Midwestern farmers. But in an area where victory came so hard no one can deny the importance of the Northwest Ordinance and Edward Coles's crusade in keeping slavery away.

A third antislavery victory of the Founding Fathers, more important than Northern abolition and the Northwest Ordinance, was the abolition of the African slave trade. This accomplishment, too often dismissed as a nonaccomplishment, shows more clearly than anything else the impact on antislavery of the Revolutionary generation. Furthermore, nowhere else does one see so clearly that Thomas Jefferson helped cripple the Southern slave establishment.

The drive to abolish the African slave trade began with the drafting of the Declaration of Independence. Jefferson, with the concurrence of Virginia and the upper South, sought to condemn King George for foisting Africans on his colonies. South Carolina and Georgia, less sure they had enough slaves, demanded the clause be killed. Jefferson acquiesced. Thus was prefigured, at the first moment of national history, the split between upper and lower South that less than a century later would contribute mightily to the disruption of the republic.

At the Constitutional Convention, as we have seen, lower South delegates again postponed a national decision on slave importations. This time a compromise was secured, allowing but not requiring Congress to abolish the trade after twenty years. A year before the deadline Jefferson, now presiding at the White House, urged Congress to seize its opportunity. "I congratulate you, fellow citizens," he wrote in his annual message of December 2, 1806, "on the approach of the period when you may interpose your authority constitutionally" to stop Americans "from all further participation in those violations of human rights which have been so long continued on the unoffending inhabitants of Africa, and which the morality, the reputation, and the best interests of our country have long been eager to proscribe." Although the law could not take effect until January 1, 1808, noted Jefferson, the reform, if passed in 1807, could make certain that no extra African was dragged legally across the seas.[13] In 1807 Congress enacted Jefferson's proposal.

The new law, although one of the most important acts an American Congress ever passed, did not altogether end African importations. Americans illegally imported approximately one thousand blacks annually until 1860. This is, however, a tiny fraction of the number that could have been imported if the trade had been legal and considered legitimate. Brazil imported over a million and a half slaves from 1807 to 1860, and the Deep South's potential to absorb bondsmen was greater. South Carolina alone imported ten thousand blacks a year in the early nineteenth century, before the law of 1808 went into effect. Louisiana creole planters sought unsuccessfully to

[13] James D. Richardson, ed., *A Compilation of the Messages and Papers of the Presidents* (Washington, 1910), 1: 396.

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make Jefferson's administration grant them the same privilege.[14] The desire of Virginia slaveholders to keep slave prices high no doubt helped feed the abolition of the trade, just as the desire of Illinois nonslaveholders to keep out blacks helped give Edward Coles his triumph. In both cases, however, the Revolutionary generation's conception of slavery as a moral disaster was of undeniable significance.

The law that closed the trade and saved millions of Africans from servitude on new Southwestern plantations also aided slaves already on those plantations. The great Southwestern boom came after the close of the African trade. Slaves could not be "used up," no matter how fantastic yearly profits were, for the restricted supply kept slave prices high. By mid-nineteenth century, moreover, almost all blacks were assimilated to the Southern way, making possible a paternal relationship between master and slave that could ease exploitation. One does not have to romanticize slave life or exaggerate planter paternalism to recognize that bondage would have been crueler if millions of Africans had been available in Mississippi and Louisiana to escalate profits. The contrast with nineteenth-century South America, where the trade remained open, makes the point with precision. Wherever Latin Americans imported so-called raw Africans by the boatload to open up virgin territories, work conditions reached a level of exploitation unparalleled in the New World. Easy access to fresh recruits led to using up laborers; and the fact that slaves were unassimilated foreigners precluded the development of the kind of ameliorating relationship that was possible between master and bondsman in North America.[15]

The law profoundly affected North American whites as well as blacks. Most notably, it shut off the South's importation of labor during the period when immigrants were pouring into the North and the two societies were locked in mortal combat. If the trade had remained open, the operation of the three-fifths clause would have given the South greater congressional representation, and a massive supply of Africans might well have helped Southerners to compete more successfully in the race to Kansas and the campaign to industrialize. As it was, with the trade closed, fresh immigration fed the Northern colossus by the hour while Southerners fell ever more desperately behind.

Perhaps the most important long-run impact of closing the trade was to help push bondage deeper into the South, thereby continuing the work the Fathers had begun with Northern abolition and the Northwest Ordinance. Now that African markets were closed the new Southwest had to procure its slaves from Northern slave states. By 1860 the resulting slave drain had significantly reduced percentages of slaves and commitments to slavery throughout the border area stretching from Delaware through Maryland and

[14] Philip D. Curtin, *The Atlantic Slave Trade: A Census* (Madison, 1969).

[15] See the judicious remarks in C. Vann Woodward, *American Counterpoint: Slavery and Racism in the North-South Dialogue* (Boston, 1971), 97–106.

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Kentucky into Missouri. Whereas in 1790 almost 20 per cent of American slaves lived in this most northern tier of border slave states, the figure was down to 10 per cent and falling by 1860. On the other hand, in 1790 the area that became the seven Deep South states had 20 per cent of American slaves and by 1860 the figure was up to 54 per cent and rising. During the cotton boom the shift was especially dramatic. From 1830 to 1860 the percentage of slaves in Delaware declined from 4 to 1 per cent; in Maryland from 23 to 13 per cent; in Kentucky from 24 to 19 per cent; in Missouri from 18 to 10 per cent; and in the counties to become West Virginia from 10 to 5 per cent.[16]

By both reducing the economic reliance on slavery and the psychic fear of blacks this great migration had political consequences. Antislavery politicians, echoing Hinton R. Helper's appeals to white racism, garnered thousands of votes and several elections, especially in Missouri, during the 1850s.[17] It was only a beginning, but it was similar to the early stages of the demise of slavery in New York.

While the end of the slave trade indirectly drained slaves from the border South another Revolutionary legacy, the tradition of individual manumissions, further weakened the institution in the Northern slave states. Although Jefferson did not live up to his dictum that antislavery planters should free their slaves many upper South masters followed precept rather than example in the antebellum years. The Virginia law of 1806, forcing freed slaves to leave the state in a year, did not halt the process as absolutely as some have supposed. Virginia laws passed in 1819 and 1837 allowed county courts to grant exceptions. The ensuing trickle of manumissions was a festering sore to the Virginia slave establishment.[18]

Meanwhile, in two border states, manumission sabotaged the institution more insistently. Delaware, which had 9,000 slaves and 4,000 free blacks in 1790, had 1,800 slaves and 20,000 free blacks in 1860. Maryland, with 103,000 slaves and 8,000 free blacks in 1790, had 87,000 slaves and 84,000 free blacks in 1860. These two so-called slave states came close to being free Negro states on the eve of Lincoln's election. Indeed, the Maryland manumission rate compares favorably with those of Brazil and Cuba, countries that supposedly had a monopoly on Western Hemispheric voluntary emancipation.[19]

The manumission tradition was slowly but relentlessly changing the character of states such as Maryland in large part because of a final Jeffersonian legacy: the belief that slavery was an evil that must some day be ended.

[16] U.S. Census Bureau, *The Statistics of the Population of the United States: Ninth Census—Volume I* (Washington, 1872), 3–8.

[17] Helper is too often treated as a lone voice crying in the wilderness when in fact he was the man who summed up in book form an argument heard constantly in the upper South. See, for example, the files of the St. Louis *Democrat,* Baltimore *Patriot,* and Wheeling *Intelligencer* during the 1850s.

[18] See, for example, John C. Rutherfoord, *Speech of John C. Rutherfoord of Goochland, in the House of Delegates of Virginia, on the Removal from the Commonwealth of the Free Colored Population* (Richmond, 1853).

[19] James M. Wright, *The Free Negro in Maryland, 1634–1860* (New York, 1921).

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Particularly in the upper South, this argument remained alive. It informed the works of so-called proslavery propagandists such as Albert T. Bledsoe; it inspired Missouri antislavery activists such as Congressman Frank Blair and the mayor of St. Louis, John M. Wimer; and it gnawed at the consciences of thousands of slaveholders as they made up their wills.[20] Jefferson's condemnation of slavery had thrown the South forever on the defensive, and all the efforts of the George Fitzhughs could never produce a unanimously proslavery society.

In summary, then, the Revolutionary generation found slavery a national institution, with the slave trade open and Northern abolitionists almost unheard. When Jefferson and his contemporaries left the national stage they willed to posterity a crippled, restricted, peculiar institution. Attacking slavery successfully where it was weakest they swept it out of the North and kept it away from the Northwest. They left the antebellum South unable to secure more slaves when immigrants rushed to the North. Most important of all, their law closing the slave trade and their tradition concerning individual manumissions constituted a doubly sharp weapon superbly calculated to continue pushing slavery south. By 1860 Delaware, Maryland, Missouri, and the area to become West Virginia all had fewer slaves than New York possessed at the time of the Revolution, and Kentucky did not have many more. The goal of abolition had become almost as practicable in these border states as it had been in the North in 1776. As the Civil War began, slavery remained secure in only eleven of the fifteen slave states while black migration toward the tropics showed every capacity to continue eroding the institution in Virginia and driving slavery down to the Gulf.

If the Founding Fathers had done none of this—if slavery had continued in the North and expanded into the Northwest; if millions of Africans had been imported to strengthen slavery in the Deep South, to consolidate it in New York and Illinois, to spread it to Kansas, and to keep it in the border South; if no free black population had developed in Delaware and Maryland; if no apology for slavery had left Southerners on shaky moral grounds; if, in short, Jefferson and his contemporaries had lifted nary a finger —everything would have been different. Because all of this was done slavery was more and more confined in the Deep South as the nineteenth century progressed.

No one spied these trends better than the men who made the Southern revolution of 1860–61. Secessionist newspaper editorials in the 1850s can almost be summed up as one long diatribe against Jeffersonian ideology and the policy to which it led. Committed lower South slaveholders knew the world was closing in on them at the very time the more Northern slave states could not be relied on. Seeing the need not only to fight off Republicans

[20] The Wimer-Blair position is best laid out in the St. Louis *Democrat*. See also Albert T. Bledsoe, *An Essay on Liberty and Slavery* (Philadelphia, 1856), and the ambiguities omnipresent in such upper South newspapers as the Baltimore *American* and Louisville *Courier* throughout the fifties.

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from without but also to halt erosion from within, radical Southerners applauded the movement to re-enslave free blacks in Maryland; many of them proposed reopening the slave trade so that the Gulf states' hunger for slavery could be fed by imported Africans instead of black Virginians; and they strove to gain Kansas in large part to keep Missouri.

When this and much else failed and Lincoln triumphed, lower South disunionists believed they had reached the moment of truth. They could remain in the Union and allow the noose to tighten inexorably around their necks. They would then watch slavery slowly ooze out of the border South and permit their own domain to shrink to a handful of Gulf and lower Atlantic states. Or they could strike for independence while the upper South retained some loyalty to bondage, thereby creating a confrontation and forcing wavering slave states to make their choice. This view of the options helped to inspire the lower South's secession, in part a final convulsive effort to halt the insidious process the Founding Fathers helped begin.[21]

When war came the lower South's confrontation strategy was half successful. Four of the eight upper South states seceded in the wake of Sumter. But four others remained loyal to the North. In the most Northern slave states, Delaware, Maryland, Kentucky, Missouri, and the area to become West Virginia, the slave drain and manumission processes had progressed too far. When the crunch came, loyalty to the Union outweighed loyalty to slavery. Abraham Lincoln is said to have remarked that while he hoped to have God on his side he had to have Kentucky. The remark, however apocryphal, clothes an important truth. In such a long and bitter war border slave states were crucial. If they, too, had seceded, the Confederacy might have survived. The long-run impact of the Founding Fathers' reforms, then, not only helped lead lower South slavocrats to risk everything in war but also helped doom their desperate gamble to failure.

Any judgment of the Founding Fathers' record on slavery must rest on whether the long or the short run is emphasized. In their own day the Fathers left intact a strong Southern slave tradition. The American Revolution, however, did not end in 1790. Over several generations, antislavery reforms inspired by the Revolution helped lead to Southern division, desperation, and defeat in war. That was not the most desirable way to abolish slavery, but that was the way abolition came. And given the Deep South's aversion to committing suicide, both in Jefferson's day and in Lincoln's, perhaps abolition could not have come any other way.

[21] I hope to demonstrate at length the positions outlined in the last two paragraphs in my forthcoming *History of the South, 1850–61*, to be published by Harper and Row. The best sources on fire-eater positions in the 1850s are the Charleston *Mercury*, New Orleans *Delta*, and *DeBow's Review*. The clearest statements of the connection between lower South secession and upper South wavering are in John Townsend, *The South Alone Should Govern the South* (Charleston, 1860), and Henry L. Benning, *Speech . . . November 6, 1860* (Milledgeville, Ga., 1860). For a preliminary estimate of how the same thinking affected the Virginia Secession Convention, see William W. Freehling, "The Editorial Revolution, Virginia, and the Coming of the Civil War: A Review Essay," *Civil War History*, 16 (1970): 64–72.

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This conclusion would have brought tears to the eyes of Thomas Jefferson. Jefferson wrote St. George Tucker in 1797 that "if something is not done, and soon done" about slavery, "we shall be the murderers of our own children."[22] In 1820 he saw with a prophet's eye how that murder would take place. The Missouri crisis, coming upon him like "a Firebell in the Night," almost caused him to shrink from even his own antislavery actions. The "momentous question," he knew, was the "knell of the Union," if not in his own time inevitably soon enough. "I regret that I am now to die in the belief," he wrote John Holmes,

> that the useless sacrifice of themselves by the generation of 1776, to acquire self-government and happiness in their country, is to be thrown away by the unwise and unworthy passions of their sons, and that my only consolation is to be, that I live not to weep over it.[23]

No sadder note survives in American literature than this scream of failure from one of the most successful of the Founding Fathers. The irony is that the ambiguous antislavery posture of Jefferson and his contemporaries helped place the nation, unintentionally but perhaps irrevocably, in lockstep toward the blowup. In the late eighteenth century a statesman had two ways to lessen the chance of a civil war over slavery. He could ease the racial, sexual, and materialistic fears that made the lower South consider emancipation anathema. Or he could scotch the antislavery idealism the slavocracy found disquieting. Jefferson, mirroring his generation and generations yet unborn, could do neither. Both his antislavery beliefs and his fear of the consequences of those beliefs went too deep. He was caught up too completely in America's most anguishing dilemma. The famed wolf he complained of holding by the ears was his own revolutionary tradition no less than blacks chained in violation of that tradition.

Like reluctant revolutionaries before and since, Jefferson sought to have it both ways. He succeeded, as such men will, in starting something destined to get out of hand. He helped protect slavery where it was explosive and helped demolish it where it was manageable. Meanwhile, he helped give informal sanction to the lower South's worst racial fears at the same time that he helped intensify those fears by unintentionally driving more blacks toward the tropics. Over a seventy-five year period the Founding Fathers' reforms added claustrophobia to a lower South psyche inflamed enough in 1787. When that happened the day of the soldier was at hand.

If in 1820 Jefferson pulled back shuddering from the horror he saw ahead, his imperfect accomplishments had taken on a life of their own. And less than a half century later, though hundreds of thousands lay slain by bullets and slaves were but half freed, mournful bells in the night would herald the realization of his most radical dream.

[22] Jefferson to St. George Tucker, Aug. 28, 1797, in Ford, *Works of Jefferson*, 8: 335.
[23] Jefferson to John Holmes, Apr. 22, 1820, in *ibid.*, 12: 158–60.

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The Cambridge History of the Bible

Reviews by

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Volume 1: *From the Beginnings to Jerome.* Edited by P. R. ACKROYD and C. F. EVANS. New York: Cambridge University Press. 1970. Pp. x, 649. $14.50.

Preface.

Language and Script: Matthew Black, The Biblical Languages. David Diringer, The Biblical Scripts.

Books in the Ancient World: D. J. Wiseman, Books in the Ancient Near East and in the Old Testament. C. H. Roberts, Books in the Graeco-Roman World and in the New Testament.

The Old Testament: Peter R. Ackroyd, The Old Testament in the Making. G. W. Anderson, Canonical and Non-Canonical. Shemaryahu Talmon, The Old Testament Text. G. Vermes, Bible and Midrash: Early Old Testament Exegesis.

The New Testament: C. F. Evans, The New Testament in the Making. R. M. Grant, The New Testament Canon. J. N. Birdsall, The New Testament Text. C. K. Barrett, The Interpretation of the Old Testament in the New.

The Bible in the Early Church: R. P. C. Hanson, Biblical Exegesis in the Early Church. M. F. Wiles, Origen as Biblical Scholar. M. F. Wiles, Theodore of Mopsuestia as Representative of the Antiochene School. H. F. D. Sparks, Jerome as Biblical Scholar. Gerald Bonner, Augustine as Biblical Scholar. J. A. Lamb, The Place of the Bible in the Liturgy.

Bibliography. Abbreviations. Notes on the Plates. Plates. General Index. Index of Biblical and Other References.

THE FIRST VOLUME of *The Cambridge History of the Bible* is primarily concerned with the composition, texts, translations, collection, and canonization of the Biblical books, and secondarily with the history of the techniques of "exegesis" (ecclesiasticese for "explanation"). The results of exegesis are sometimes mentioned. The devotional use of the Bible is recognized by one chapter on the Bible in the liturgy. The Bible in law (Jewish and Christian), the Bible in art, the Bible in medicine and magic go unnoticed, but there are two introductory chapters on "Biblical" languages and scripts and two on books in the ancient Near East and the Greco-Roman world.

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The work is evidently intended for the better Bible teacher. The articles, though written by scholars, avoid scholarly discussions and try to state what the authors wish to be "the generally accepted facts." Footnotes are few and bibliographies skimpy. When an author does diverge from the familiar it is usually in favor of one of his own theories, or for apologetic purposes. The apologetics are mostly of the "pseudorthodox"[1] variety; that is to say the authors do not hold the orthodox positions but try to diverge from them as little and as inconspicuously as possible. Embarrassing facts are avoided when they can be, admitted when they must be, minimized when admitted, and excused even when minimized. Thus, with a little special pleading here, a slight overstatement there, and a bit of suggestion somewhere else, the following picture emerges.

The Israelites were Aramaeans (as Old Testament legend says) before they conquered Palestine, but with the conquest they began to speak Hebrew,[2] and from then through New Testament times were a peculiar people with a peculiar language, the instrument of a peculiar revelation.[3] "Moses himself, tutored at the Egyptian court"—the proof of this is Acts 7:22—"is said to have recorded laws,"[4] and the officials (*shoterim*) whom he is said to have appointed must have been literate because a cognate root in Akkadian means "to write."[5] Given such beginnings by men who wished to express their "consciousness" of "the workings of God in Israel's affairs,"[6] "we are bound to take into account . . . the possibility that . . . [the compilations that make up the present Old Testament books] were produced as a result of the activity of men of genius."[7] After production they were often interpolated because of the conviction that "prophetic revelation . . . is a continuous process into the present."[8] Gradually, however, they attained such acceptance and authority as to constitute a closed canon of sacred texts with which further tinkering was impossible. This "Palestinian canon"—the present Hebrew Bible—"had become so firmly established in Judaism

[1] For "pseudorthodoxy" and its importance in contemporary Biblical criticism see Morton Smith, "The Present State of Old Testament Studies," *Journal of Biblical Literature,* 88 (1969): 19–35.

[2] See Black, "The Biblical Languages," 1. There is no good evidence for the change of language.

[3] Black suggests the term "Holy Ghost Language" for "Biblical Greek." *Ibid.,* 11 n.2.

[4] Diringer, "The Biblical Scripts," 37. No doubt in hieratic.

[5] *Ibid.,* 37 n.2.

[6] Ackroyd, "The Old Testament in the Making," 91. For "consciousness" read "delusion." It is worth noting that the critical passages in Ackroyd's text refer to the Israelite deity as "YHWH," the homiletic passages as "God." The source critic will also find passages (redactional?) in which both divine names occur, for example: "Canaanite sanctuaries became hallowed as Israelite places of worship, associated now with a personal revelation of God under his name YHWH" (p. 70).

[7] *Ibid.,* 105. Ackroyd says nothing of the possibility that they were produced by pious lunkheads, though these are more common than men of genius, more often write religious books, and have been plausibly hypothecated to explain the inconsistencies and absurdities of many Old Testament texts.

[8] Anderson, "Canonical and Non-Canonical," 127. Nothing is said of the conviction that desired results can be got by circulating bogus prophecies.

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even before its formal ratification in the Jamnia period, that it was automatically accepted by the Christian community."[9] It was at that time represented by three main families of texts—Septuagintal, Samaritan, and proto-Massoretic[10]—and was already the object of exegetic tradition of which the beginnings had been coeval with the texts themselves.[11] "Scholars not misled by the analytical tendency of the literary-critical school will fully appreciate the importance of primitive midrash [the earliest rabbinic exegesis] to a proper understanding of the spirit in which scripture was compiled."[12]

The New Testament began from the Old, which was the early Christians' "only literature"[13] and whose prestige long inhibited them from writing. When they did start to write they wrote artlessly and *ad hoc:* Revelation is "the only New Testament book which appears to have been written self-consciously as if for canonical status"[14] and, indeed, is "the only disciplined artistic work in the New Testament."[15] Nevertheless, some of the native woodnotes woolly of early writers—at least the major Pauline epistles and the Synoptics—were "accepted" by the "embryonic orthodoxy" that, it is suggested, prior to the Gnostic debates of the second century included "most Christians."[16] From then on "the primary criterion" in canonization "was traditional usage among groups known to have held the traditional faith."[17] While apostolic authorship was a mistaken assumption, the Christians "were right in regarding the accepted books as expressions of the apostolic spirit."[18] The texts of the books canonized were even before canonization represented by several different families, therefore we cannot hope by mere comparison of manuscripts to get back to the original text; there is nothing to do but construct an eclectic text by "rational criticism."[19]

[9] *Ibid.*, 159 (in spite of the evidence to the contrary from the Samaritans, the Sadducees, Josephus, the Qumran material, and early Christian usage). Anderson recognizes the possibility that "the third section of the Palestinian canon was . . . somewhat nebulous" before Jamnia, but favors the view presented in the text.

[10] The student should not suppose that in a modern Hebrew Bible he gets a critical text based on all this evidence. The text of the standard "critical" edition of the Hebrew Bible is a transcription of a single medieval manuscript. Volume 1 of *The Cambridge History of the Bible* does not mention this fact.

[11] Vermes, "Bible and Midrash: Early Old Testament Exegesis," 214.

[12] *Ibid.*, 228–29.

[13] Evans, "The New Testament in the Making," 232. Deliberate neglect of the extracanonical Jewish literature—of which both the New Testament and the later books of the Old Testament were originally parts—is an even more serious fault than the supposition that the early Christians were ignorant of all Greek and Latin literature. Aratus is quoted in Acts 17:28; "Epimenides" in Titus 1:12; Enoch in Jude 14.

[14] *Ibid.*, 234. But the pseudonymity of the Pastorals and 2 Peter suggests canonical pretensions; John, Hebrews, Ephesians, and 1 Peter were obviously written to be "holy scriptures."

[15] *Ibid.*, 281. He again forgets Hebrews and the like.

[16] Grant, "The New Testament Canon," 286. But it seems at least equally likely this "embryonic orthodoxy" was a small minority of the earlier worshipers and/or followers of Jesus.

[17] *Ibid.* For "groups known to have held the traditional faith" read "groups that generally agreed with each other."

[18] *Ibid.*, 307. How this spirit can be recognized if there are no apostolic works is not explained. The problem is made more difficult by the fact that the canonical books contradict each other—or rather, in the words of Professor Grant, "attest the rich diversity of early Christian thought" (p. 307).

[19] Birdsall, "The New Testament Text," 309–17. This section should be required reading for editors of the text of the Old Testament.

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Most authors of the New Testament "regarded the Old Testament as an authoritative body of literature which claimed the attention and obedience of all Christians."[20] They inherited the Jewish methods of interpreting it[21] and thus found in it important confirmation of positions of whose truth they were already satisfied on other grounds.[22] They also found assorted passages that they took to be prophecies of Jesus' adventures, and their use of these "is surprisingly appropriate."[23] Reference to such passages was "the earliest method of evaluating the theological significance" of Jesus' life.[24] Hence it might be thought natural for New Testament texts to be given the same treatment, but this in fact seems first to have been done "among the heterodox gnostic sects . . . and . . . orthodox Christian writers only adopted the allegorisation of the New Testament by way of defence."[25] Once adopted, the practice led to regrettable misunderstandings, as did the later Christian attitude to the Bible as an essentially oracular work. Allegorization, however, must be carefully distinguished from the correct contention that the Bible often speaks in figurative language and is not to be taken literally.[26] Moreover, even allegorization itself was excusable because, to construct a consistent body of doctrine "emancipation from a very literal and pedestrian observance of the text of the Bible was not only commendable but essential."[27] The Fathers, "frequently wrong about the details, . . . were usually right about the end result,"[28] and, anyhow, "they inaugurated the Church's dance with the Bible"—the type of dance is not specified, and one thinks of those modern sorts in which the partners turn their backs on each other and bump. Of this curious performance the works of Origen, Theodore of Mopsuestia, Jerome, and Augustine serve as examples. Origen's allegorization is recognized but excused—had he not resorted to allegoriza-

[20] Barrett, "The Interpretation of the Old Testament in the New," 377. This gives a false impression: most of the New Testament was written before "the Old Testament" was either a definite body of literature or "the Old Testament."

[21] About these Barrett is sometimes far at sea; see, for example, p. 385: "when the Hellenistic methods" of exegesis "were first adopted, about 100 to 25 B.C. . . . the borrowing took place in the best period of Talmudic jurisprudence"!

[22] *Ibid.*, 397, 399.

[23] *Ibid.*, 401. His expectations must have been low.

[24] *Ibid.*, 405. But it is at least as likely that the cult of a supernatural miracle worker came first; the Old Testament interpretations are often demonstrably secondary.

[25] Hanson, "Biblical Exegesis in the Early Church," 416. For this he has one example.

[26] *Ibid.*, 447. The consequences of this sound principle are: first, in particular, the Arians were mistaken in supposing that because the Bible described God as father and Christ as son it followed that Christ was nonexistent before he was begotten; second, in general, Biblical statements are to be revered but not believed, except when they happen to confirm orthodox or contradict heretical positions. Athanasius' statement, "Even if it is impossible to grasp what God is, yet it is possible to say what he is not . . . to condemn the suggestions of the heretics" (*Epistula ad monachos* 1.2) is quoted with approval (p. 448).

[27] Hanson, "Biblical Exegesis in the Early Church," 449. The desirability of a consistent structure based on misinterpreted evidence is taken for granted, and this end justifies the means.

[28] *Ibid.*, 453. The proof of this is that the Athanasian doctrine was more faithful to "the New Testament account of the significance of Jesus Christ" than was the Arian (p. 453). (There is no one New Testament account of Jesus Christ, and a number of the various statements about him—notably those in Paul and the Synoptics—fit an adoptionist or subordinationist Christology better than they do the Athanasian; see also p. 444.)

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tion "he would have to regard" some Biblical texts as false or immoral.[29] Theodore[30] is gently rebuked for his historical "approach which, however sensible and shrewd its comments, tends all too often to fall short of grasping the eternal dimension at the heart of the Gospel."[31] Of Jerome as translator, text critic, and commentator there is an excellent account that says almost nothing of the theological concerns of his exegesis. Of Augustine we have a determined defense; he was converted, as he claims, by realizing that Ambrose's allegories revealed the true sense of the texts.[32] His arbitrary interpretations are "explained" by the fact that for him Holy Scripture was inspired by the voice of God.[33] His typological exegesis, inspired by Tyconius, was of a type that "avoided crude literalism on the one hand and overstrained allegory on the other."[34] His exegetic principle that Scripture cannot contain deliberate falsehood because that would have bad consequences—it would open the door to individual interpretation and so undermine ecclesiastical authority—is defended as "a plea for honesty in the Christian exegete, for the sort of attitude which makes serious criticism possible."[35] Finally, Lamb's "The Place of the Bible in the Liturgy" goes through the services one by one, describes the Biblical material used in each, and closes with a defense of their use: "As A. G. Hebert said, 'It is not that the prophets were inspired . . . to foretell various details in the life of Jesus; it is that the prophets . . . give symbolical and poetical expression to great theological principles which find their full embodiment in Jesus the Messiah.' "[36] It remains only to notice what is perhaps a printer's error: at the end of the book the word "Amen" has been omitted.

[29] Wiles, "Origen as Biblical Scholar," 463. The pseudorthodox reader will understand that this justifies allegory.

[30] The choice of Theodore rather than the far more influential Chrysostom is justified by the fact that "Chrysostom is . . . the preacher who makes use of . . . biblical interpretations; Theodore is . . . biblical scholar and commentator" (p. 490). This is typical of the book—a history of the Bible as copied and as studied, not as used.

[31] Wiles, "Theodore of Mopsuestia as Representative of the Antiochene School," 506.

[32] Bonner, "Augustine as Biblical Scholar," 543. Can this be true? Augustine was no fool, and Ambrose must also have revealed, to an ambitious man, how powerful a bishop could become. Of course we have the *Confessions* to the contrary. But can the passionate published confessions of a professional rhetorician be doubted?

[33] *Ibid.*, 547. But this does not explain why he should have been indifferent as to just what the voice of God did say—that is, the precise reading of the text—nor why he should have thought that it meant whatever came handy.

[34] *Ibid.*, 555. For "crude" read "accurate." The distinction between nonsense and overstrained nonsense is overstrained.

[35] *Ibid.*, 556. It would be more honest to admit that some of the "sacred authors" were liars—for example, some editors of Chronicles, and the authors of the pseudonymous "Pauline" epistles.

[36] P. 585. Hebert's words are cited from *Liturgy and Society*, (London, 1935), 216. They are false both to the New Testament and to the facts. In the New Testament the prophets are repeatedly cited as having foretold "various details in the life of Jesus"—that his garments would be divided and that lots would be cast for his chiton (John 19:24); that none of his bones would be broken (John 19:35–36); and that his enemies would watch his agony (John 19:37). As to the facts: the prophets are poor in "great theological principles" and some of those they do express—for example, that punishment is to be exacted only from the individual who sins (Ezek. 18:20)—are contradicted in the New Testament.

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It would be unfair to close this review with the preceding sentence. Apart from the slant and the skimping of scholarly discussion and apparatus, the individual chapters—with few exceptions[37]—give reasonably good and up-to-date accounts of the subjects discussed. Birdsall and Talmon on the New and Old Testament texts are particularly good; next come Sparks on Jerome and Wiles on Origen and Theodore. Roberts on books in the Greco-Roman world is written from a mastery of the subject that makes the reader wish for a fuller treatment. Ackroyd on "The Old Testament in the Making" approaches with prudence a difficult task; the principles of criticism stated at the beginning of the chapter are admirable. Almost every chapter has some good observations.[38] All this is what one would have expected in a work organized and written mostly by distinguished British scholars.

Indeed, one would have expected more. The most surprising thing about this history of the Bible is its general neglect of the connection of the Biblical books with history. The historical circumstances that produced the books are slighted;[39] the historical consequences they produced are almost totally ignored. The history of the Bible is one of the great tragedies of the Western world—and typically Western because it is in part a tragedy of superannuation consequent on social change. The laws, legends, and history of a comparatively primitive people, their prophets' visions of their god and of what he would do, the stories and sayings of a miracle worker and of his early followers, and the letters and visions of some of these followers who expected him to return on the clouds to judge the quick and the dead—all these were accepted as divine truth and became the sacred literature of Western society: perhaps the most important element in the intellectual, moral, and artistic environment of more than a thousand years. The particular contents of the individual books were to have enormous impact on almost every aspect of society; the history of Western thought in

[37] Wiseman's chapter on books in the ancient Near East is easily the worst in the volume. It is oblivious to the difference between cuneiform writing (a learned profession) and alphabetic writing (a common skill) and consequently to the differences between the roles of scribes and writing in Bronze Age Mesopotamia and Iron Age Palestine. In the latter "the cheapest and most durable writing material" was not (as Wiseman thinks, p. 32) a clay tablet, but a potsherd. Yet ostraca—of fundamental importance for Old Testament history—get just two sentences, whereas clay tablets—rare and unimportant in Biblical Palestine—get two pages. Add a totally uncritical acceptance of Biblical legends and apologetic arguments. The result is one more demonstration of how much the study of ancient Mesopotamia can contribute to the misunderstanding of the background of the Bible. Runners up—or should one say, down?—are Vermes, Bonner, and Black.

[38] For example Anderson's that the concept of "canonicity" differs from time to time and community to community (p. 113); Evans on the peculiarity of the Pauline "epistle" as a literary form (p. 237); Grant's note that as a result of the means of selection and preservation "our information" about the growth of the canon "has a built-in bias towards orthodoxy and uniformity" (p. 295); Barrett's recognition that the primary source of authority in the early Christian communities was often not the Old Testament (p. 399); Hanson on the Christian attitude to the Bible as an oracular work and the consequences of this (419–22), also on Christian denunciation of pagan allegorizing (p. 429).

[39] The especially unfortunate neglect of the apocrypha and pseudepigrapha has already been mentioned.

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particular was to be largely that of a struggle with this complex of power and of falsity, at once inspiration and incubus. How and why this complex arose, was organized, and became dominant in Western society is the proper theme of this volume, but one would never know that from the text. I wonder why.

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Volume 2: *The West from the Fathers to the Reformation*. Edited by G. W. H. LAMPE. New York: Cambridge University Press. 1969. Pp. ix, 565. $12.50.

Preface.

The Rev. Professor Bleddyn J. Roberts, The Old Testament: Manuscripts, Text and Versions. The late C. S. C. Williams, The History of the Text and Canon of the New Testament to Jerome. T. C. Skeat, Early Christian Book-Production: Papyri and Manuscripts. The late Fr. E. F. Sutcliffe, S.J., Jerome. Raphael Loewe, The Medieval History of the Latin Vulgate.

The Exposition and Exegesis of Scripture: The Rev. G. W. H. Lampe, To Gregory the Great. Dom Jean Leclercq, O.S.B., From Gregory the Great to St. Bernard. Beryl Smalley, F.B.A., The Bible in the Medieval Schools. The Rev. S. J. P. van Dijk, O.F.M., The Bible in Liturgical Use. Erwin I. J. Rosenthal, The Study of the Bible in Medieval Judaism.

The Very Rev. R. L. P. Milburn, The "People's Bible": Artists and Commentators. Professor Francis Wormald, Bible Illustration in Medieval Manuscripts.

The Vernacular Scriptures: M. J. Hunter, The Gothic Bible. Geoffrey Shepherd, English Versions of the Scriptures before Wyclif. Henry Hargreaves, The Wycliffite Versions. W. B. Lockwood, Vernacular Scriptures in Germany and the Low Countries before 1500. C. A. Robson, Vernacular Scriptures in France. Kenelm Foster, O.P., Vernacular Scriptures in Italy. Margherita Morreale, Vernacular Scriptures in Spain.

Fr. Louis Bouyer, Erasmus in Relation to the Medieval Biblical Tradition.

Bibliography. Notes on the Plates. Plates. Indexes.

THE SECOND VOLUME of *The Cambridge History of the Bible* is, on the whole, well structured. The main sections (chs. 5–9) are those on the tradition of the text of the Vulgate, on medieval exegesis, on the liturgical function and the illustrations of the Bible, and on the vernacular versions. These sections are preceded by introductory chapters on the transmitted Hebrew version—Masora—and on the establishment of the canon, on early Christian book production, Jerome, and on early Christian exegesis, and are followed by a concluding chapter on Erasmus. The extensive introduction contains, on the one hand, materials of more than an introductory nature (ch. 3), while, on the other hand, it lacks a coherent discussion of the legal exegesis and the allegorical-homiletic exegesis in the ancient Jewish tradition (midrash halakah, midrash haggada). There is no mention of the

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exegetical "measures"—rules of interpretation (*midot*)—or of their possible influence on Christian tradition, on the seven rules of Tyconius, for example.[1] Only Jerome and Erasmus are given relatively extensive intellectual biographies—a decision that almost suggests a reading of the medieval history of the Bible as beginning and ending with the personalities of the two extraordinary humanists. Later scholasticism is excluded as *terra incognita* (p. 219).

Being more at home with the exegetical history than with the other topics dealt with in the book, I will confine most of my remarks to the sections dealing with the interpretative endeavors of the Middle Ages. The chapters on text history by Bleddyn J. Roberts, the late C. S. C. Williams, and Raphael Loewe are concise and comprehensive. In the discussion of the Masora the testimony of the Qumran fragments is carefully considered; it confirms the early beginnings of the Masoretic text. The identification of the Masora with normative Judaism ("hierarchical control") and of the popular versions with the sectarian (p. 4) is somewhat schematic. Popular versions are at times possibly nothing but texts used for other than liturgical purposes; neither could we construe an ideological ground for those variations closer to the Septuagint. The problems involved in the early history of the New Testament canonization, as for example the role of Marcion (pp. 44–47), are carefully argued.

In the fourth chapter E. F. Sutcliffe stresses the accuracy of Jerome's translations "once the characteristics of his translations are taken into account" (p. 97). The author's discussion of Jerome's *spiritualis intelligentia* does not tie Jerome to the exegetical traditions and thus remains vague. Nor do we hear about Jerome's ambivalent attitude toward the classical tradition. That antiquity lacked a "science of etymology" (p. 100) is about as true as saying that it lacked a science of physics. The classical etymological systems were born out of the controversy over whether language exists by nature (*phúsei*) or by convention (*thései*), but the positions taken could not remain dogmatically disjunctive for long. Even the defendants of the logical-natural structure of language came to concede, as did Varro, that some words changed their form or their connotation, and that some meanings, particularly of words of foreign origin, might have been lost.[2] Perhaps not even this modicum of historical insight could be admitted into the *sensus litteralis*—despite internal references to change of habits of speech, as in

[1] Of the rules of Tyconius, the fourth (*de recapitulatione-ein mukdam u'meukhar*) and the sixth (*genus et specie-kelal u'perat*) may be, except for the number of the rules itself, a reminiscence of the *midot*. See Traugott Hahn, *Tychonius-Studien*, 2 (Leipzig, 1900): 21.

[2] Varro *De lingua Latina* 5. 1. 1–10 (ed. G. Goetz and F. Schoell [Leipzig, 1909], 4–6). Varro's observations on the limits of etymology were transmitted to the Middle Ages by Isidor of Sevilla, *Etymologiae sive origines* 1. 29 (ed. W. M. Lindsay [Oxford, 1911]). From the vast literature on the subject, H. Steinthal, *Geschichte der Sprachwissenschaft beiden Grichen und Römern* (Berlin, 1890; reprint, Hildesheim, 1961) is still useful. See also Varro *De lingua Latina* 1. 343–44; 2. 130; and the recent summary in Max Pohlenz, *Die Stoa* (Göttingen, 1959) 1: 37; 2: 21–24.

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1 Samuel 9:10—if the language of the Scriptures was to be conceived as perfect, unchanged, and therefore unambiguous.[3] Historical-philological criteria similar to those developed in the course of the Homer-exegesis were applied to the study of the Bible by pagan polemicists rather than by orthodox exegetes. Hieronymus is known to have refuted (and conserved!) Porphyrius's assessment of the book of Daniel as a *vaticinium ex eventu* composed in the time of the Hasmoneans.[4] The reader should perhaps have been told in this or in the subsequent chapters something about the contents and the impact of pagan Biblical criticism.

The short account of the formation of the allegorical and typological disciplines by G. W. H. Lampe starts with a few general remarks on Jewish exegesis. They convey the impression that the Qumranian "meaning" (*pesher*) and the rabbinical midrash share the same arbitrariness of ahistorical actualizations. Yet the *pesher*, as against admission of many possible allegorical applications of a Biblical verse in the midrash haggada, is apocalyptical and apodictical throughout, and lives by the sectarian eschatological polarization of history and even of Israel itself. The difference lies less in the technique of exegesis than in the scope of its application. Take, for example, the "interpretation of Habakkuk." Not only did Habakkuk prophesy for the present; he prophesied in reality for and about the latter-day community as the only holy community (*adat kodesh, Civitas Dei*),[5] which, by alone being able to read the prophecies properly[6] proves its claim for the present to be indeed the turn from the old to the new aeon and proves the sect to be the true remnant Israel. Here lies a real link to early Christian exegesis.

As so often in modern discussions of Christian exegesis, the differences in approach are discussed with reference only to the historical narrative (events, persons) or to prophecy. The Christian understanding of the Old Testament was an attempt by men caught in the dialectics of conflicting polemical needs to determine the nature of the continuity from the old to the new dispensation. No less significant than its interpretation of history and prophecy was therefore its exegesis of the Mosaic law. Was it a negative or positive preparation of Christianity? Was it from its very beginning incomprehensible and unfulfillable, given as a mere burden and punishment, or did it have a concrete historical function? The difference between the

[3] Hieronymus conceded, however, that the names of nations and tribes of the genealogical lists in Genesis might have changed. *Hebraica questiones in Genesim* 10. 26–29, in *Opera I*, 1, ed. P. de Lagarde, in Corpus Christianorum Series Latina 72. 1. 1: 14. On his attitude toward the Hebrew language, as against that of the Antiochians, who believed Hebrew to be a product of *Meinungen über Ursprung und Vielfalt der Sprachen und Völker* (Stuttgart, 1957) 3: 1, 385–91.

[4] Adolf von Harnack, *Porphyrius, "Gegen die Christen"* (Berlin, 1916), vol. 1; Otto Eissfeldt, *The Old Testament: An Introduction*, tr. P. R. Ackroyd (Oxford, 1965), 517; Elias Bickermann, *Der Gott der Makkabäer* (Berlin, 1937), 143.

[5] David Flusser, "The Dead-Sea Sect and Pre-Pauline Christianity," *Scripta Hierosolymitana*, 4 (1958): 227.

[6] See Matt. 13:15 and Ps. 78:2 as against the rabbinical use of *mashal lema hadavar dome*.

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typological-historical approach of the Antiochian school and the radical allegorization of the Alexandrians bears on this point also. The Alexandrinian tradition tended to minimize the unique historical role of Israel "in the flesh" within the economy of salvation, while it is interesting to note how some Antiochians stress the positive-historical, antipolytheistic function of the ceremonial law in a language resembling certain exegetical trends in the midrash.[7] These traditions can be traced both in the Greek and the Latin exegesis down to the Middle Ages and beyond.

Lampe's article concentrates on the differences between the Antiochian and Alexandrinian traditions. The Latin reception of both is dealt with most summarily, although the function of the article would have suggested a reverse emphasis. The traces of the Antiochian tradition in the West are confined to a discussion of the typological method of Tyconius and Augustine. Junilius Africanus deserved mention. The process by which the medieval "fourfold levels of meaning" (*quattuor sensus*) assimilated elements of both the Alexandrinian and Antiochian traditions is not made as clear as it is, for example, in the short description of Latin exegesis in later antiquity given by Herbert Grundmann.[8]

Dom Jean Leclercq, in a most erudite article on exegesis from Gregory to St. Bernard, emphasizes the role of monastic reforms in the exegetical history of the early Middle Ages and seems to overemphasize St. Bernard's importance as an exegete. The chapter is by far too short, at times a mere enumeration of celebrities (see p. 187). In such a limited space the author could hardly pay attention to the intellectual or political context of Biblical interpretation—for example, the Carolingian intellectual climate or, more important, the role of Biblical arguments in the investiture struggle. What Reinhold Seeberg, following Albert Hauck, called the "speculative biblicism," or what Alois Dempf called the "symbolism," of the twelfth century,[9] as expressed in Rupert of Deutz, Gerhoch of Reichesberg, and Hugh of St. Victor, among others, is only touched upon. Its peculiar features—the conception of all of history and not only that of the early Church as a continuous actualization of Biblical prefigurations and thus as part of the exegete's task; the search for "concordances"—are not discussed. They were to lead to Joachim's *concordia*, the attempt to grasp the totality of history as a threefold repetition on an ever higher level—the period of the Father, the Son, and of the Holy Spirit—of analogous events. Nor is Joachim of Fiore himself ever discussed in the entire book. This is an editorial omis-

[7] Theodoret of Cyrrhus *Quaestiones in Leviticum* (Jacques Paul Migne, ed., *Patrologiae cursus completus. Series Graeca* [hereafter *PG*] [Paris, 1857–1912], 80: 300); see also *PG*, 83: 991; compare with *Vajikra Rabba* 22. 6 (ed. Mordechai Margulies [Jerusalem, 1956], 3. 517).

[8] Herbert Grundmann, *Studien über Joachim von Fiore* (Leipzig, 1927; reprint Darmstadt, 1966), 23–40.

[9] Reinhold Seeberg, *Lehrbuch der Dogmengeschiche* (Berlin, 1930; reprint Darmstadt, 1959) 3: 184 following Albert Hauck, *Kirchengeschichte Deutschlands* (reprint Berlin, 1958) 3: 434–45; Alois Dempf, *Sacrum Imperium* (Munich, 1929), 229–68.

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sion;[10] the articles were all so well defined in their scope that Joachim and his deep impact on medieval exegesis succeeded in evading everyone's jurisdiction.

Beryl Smalley's article on the Bible in the medieval schools is a most lucid summary of her famous book. It shows the *Sitz im Schulleben* of medieval exegesis. It shows also that it is one tradition leading from the Victorine revival of the *sensus historicus* to Thomas Aquinas's assertion that the whole original meaning of the Biblical words, including the intended parables and allegories, are to be seen as part of the literal sense, while only the events or things signified by them may again have a deeper signification.[11] Attention could also be drawn to the link between Thomas's theory of signification as the background of both his exegetical theory and the theory of permissible equivocation underlying the concept of *analogia entis*. The author rightly draws attention to Thomas's philosophy, his "stress on the human agent of revelation" (p. 215) corresponding, one could add, to the stress Thomas lays on spontaneous human ingenuity (*adinventio humanae rationis*) in actualizing natural law.

The chapter on the Bible in liturgical use by S. J. P. van Dijk, O.F.M., is one of the best in the book. It should perhaps have been followed by a chapter on the homiletical functions and uses of the Bible. The last chapter in this section, by Erwin I. J. Rosenthal, is devoted to medieval Jewish exegesis; because of the context of his discourse he gives special attention to Jewish-Christian polemics and mutual influences. The separation between controversy and reception is somewhat artificial; Rashi's exegesis proves it. Rashi's interpretation of the "servant of God" in Deuteroisaia (Isa. 53) accepts a Christian theologoumenon in order to reverse its application. The reception of the Christian *topos* of the "ages of the world" (*aetates mundi*) as prefigured in the days of creation by Abraham bar Chija and by Nachmanides in order to disprove the messiahship of Christ is another example. The influence of Rashi on Christian exegesis is not without an ironic point. Christian exegesis, when it felt the need to return to the *fundamentum fundamenti,* looked for it in the *veritas hebraica,* which it dogmatically supposed to have always been the only way in which the Jews could explain their scriptures, namely *secundum carnem;* little did these Christian exegetes know that the revival of the historical-philological sense was almost as much a recent revival among Jews as it was in Christian scholarship. The influence of Maimonides' philosophical allegoresis on Thomas is mentioned, as is the influence of Maimonides' doctrine of "the reasons for the commandments" (*ta'ame ha'mitsvot*) on Thomas's *rationes preceptorum*. It could have been made clear how revolutionary (and therefore contested) this historical hermeneutics was. Maimonides was

[10] A similar observation was made by J. S. Preus in a review in *Speculum,* 45 (1970): 307.

[11] Thomas Aquinas *Summa Theologica* 1. 1. 10. See also Alexander of Hales *Summa Theologica* 2. 3. 2. (Quaracchi, 1948), 4: 760.

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one of the very few thinkers before the historical revolution of the sixteenth and seventeenth centuries to have foreshadowed the method that I have called elsewhere "understanding by alienation and reconstruction." Much as Cujas or Hotman insisted on the irrelevance for their society of the *Corpus Juris Civilis*,[12] since it reflects not eternal norms but an alien, ancient context of institutions, life, and thought, of which even those parts that survived can only be understood after careful reconstruction of their original setting, so also Maimonides insists on the incomprehensibility of the ceremonial and dietary laws unless we reconstruct their original context. He found this context in the "Sabean" polytheistic culture, which these commandments were slowly to replace. In his radical historical approach, not even Christian exegesis followed Maimonides, in spite of the fact that it has always regarded Mosaic law as a *bonum in suo tempore*.[13]

Ibn Ezra, it is true, was not a Biblical critic "centuries before higher criticism" (p. 267), but his influence on Spinoza and Richard Simon was substantial. In part this is due to the fact that Ibn Ezra mentions many critical arguments, albeit in order to refute them. The commentary of Ramban (Nachmanides) is only mentioned as against the extensive discussion of Abravanel. If influence on Jewish exegesis is the criterion of such an emphasis, Nachmanides was the first to give a systematic integration of *kabbala*, philosophy, and exegetical tradition; his influence was enormous. If influence on Christian exegesis is the criterion, his impact on Meister Eckhart should have recommended him. Nachmanides insists against the radical rationalist on the importance of the haggada (as the ancient vehicle of esoteric knowledge) for the understanding of the Scriptures. Another Nachmanides confronts us at the disputation in Barcelona (mentioned here, p. 165), where he asserts categorically that the midrash, which the Dominicans used either to prove the tacit acceptance by the sages of Christian dogmas or to show the absurdities of the anthropomorphistic Jewish imagination, was only of the nature of "sermons" and therefore not binding. Any investigation into Christian-Jewish polemics in the Middle Ages must bear in mind what effect this feeling of being conspicuous, of *être vue* (to speak with Sartre), had on the Jews since the twelfth century, that is, since the post-Biblical Jewish literature became known to the Christian environment.[14] I do not agree with the remark that the burning of the Talmud,

[12] Funkenstein, "Gesetz und Geschichte: Für historisierenden Hermeneutik bei Moses Maimonides und Thomas von Aquin," *Viator*, 1 (1971): 147–78. On historical reasoning in the legal science of the sixteenth and seventeenth centuries, see the excellent summary of J. G. A. Pocock, *The Ancient Constitution and Feudal Law* (Cambridge, 1957) ch. 1; and D. R. Kelley, *Foundations of Modern Historical Scholarship: Language, Law and History in The French Renaissance* (New York, 1970).

[13] Hugh of St. Victor *De Sacramentis Christianae fidei* 2. 6. 4 (Jacques Paul Migne, ed., *Patrologiae cursus completus. Series Latina* [hereafter *PL*] [Paris, 1844–55], 176: 450a). The expression, which will occur later in Joachim of Fiore's concordantia, might have been influenced by Rashi on Gen. 6:9. See also Hugh *De Vanitate mundi* 3 (*PL*, 176: 725d). Noah was only *compartione pessimorum iustus*.

[14] Important material for the time up to the thirteenth century can be found now in the

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like the disputations, was always a "manifestation of the relentless campaign . . . to convert the Jews" (p. 264). It was, however, at times an attempt to accuse the post-Biblical Jewish exegesis—as such was the Talmud stamped—of being a *nova lex* and therefore heretical, even in the terms of Judaism itself. Christian scholarship since the thirteenth century was thus a mixed blessing. Jews and Christians learned much more about their mutual theological positions and were influenced much more by them, but this increased knowledge only widened the subjective gap. The history of exegesis could demonstrate this process.

Later medieval exegesis has been dealt with too briefly in the book. Knowing very little about medieval art or of the vernacular versions of the Bible, I am grateful for the information so well presented in these chapters. While descriptions of medieval exegesis are available,[15] I am not aware of a comparable survey of the vernacular versions. To sum up, the reader of this volume will seldom be able to discern what was continuous and what has changed in the medieval understanding and application of the Bible. Nor will he find much about the historical contexts of the medieval preoccupation with the Bible. We are, however, enriched by a useful introduction to a subject as important as it is vast and difficult.

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Volume 3: *The West from the Reformation to the Present Day*. Edited by S. L. GREENSLADE. New York: Cambridge University Press. 1963. Pp. ix, 589. $12.50.

Preface.

Roland H. Bainton, The Bible in the Reformation. Basil Hall, Biblical Scholarship: Editions and Commentaries.

Continental Versions to *c.* 1600: Hans Volz, German. Kenelm Foster, O.P., Italian. R. A. Sayce, French. S. Van der Woude, Dutch. E. M. Wilson, Spanish. R. A. Auty, The Bible in East-Central Europe. Bent Noack, Scandinavian.

S. L. Greenslade, English Versions of the Bible, 1525–1611. The late Norman Sykes, The Religion of Protestants. F. J. Crehan, S.J., The Bible in the Roman Catholic Church from Trent to the Present Day. W. Neil, The Criticism and Theological Use of the Bible, 1700–1950. Alan Richardson, The Rise of Modern Biblical Scholarship and Recent Discussion of the Authority of the Bible.

Continental Versions from *c.* 1600 to the Present Day: Bernhard Dammermann, with a note on Catholic versions by S. L. Greenslade, German. R. A. Sayce, French. S. Van der Woude, Dutch. E. M. Wilson, Spanish. Bent Noack, Scandinavian. Kenelm Foster, O.P., Italian.

Luther A. Weigle, with a note on the New English Bible by C. F. D. Moule,

descriptive rather than analytical book of Chajim Merchavia, *The Church versus Talmudic and Midrashic Literature* (Jerusalem, 1970). See also our remarks in "Changes in the Pattern of Christian Anti-Jewish Polemics in the 12th Century" *Zion*, 33 (1968): 126–44.

[15] Henri de Lubac, *Exégèse Médiévale, Les quatre sense de l'écriture* (Paris, 1959–61).

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English Versions since 1611. Eric Fenn, The Bible and the Missionary. M. H. Black, The Printed Bible. S. L. Greenslade, Epilogue. D. R. Jones, Appendices: 1) Aids to the Study of the Bible, 2) Commentaries.

Bibliography. Notes on the Plates. Plates. Indexes.

"THE LAST SHALL BE FIRST." This Biblical dictum applies to one of the most auspicious books published about the Bible in the modern English-speaking world. This volume, which deals with the third chronological part in a sequence, appeared first in 1963. Whether this modern or "last" work shall also be regarded as critically first and best of the three we shall have to leave to others to judge, though few people could be expected to be sufficiently at home in the various eras of Biblical production and interpretation to render such a comparative judgment. Volume 3 stands on its own merits, and the eight years since publication have provided many opportunities for users to test the book's durability against the lavish welcome given it by the first critics who appraised it.

Concerning the book's physical durability there need have been no doubt. While reviewers seldom feel obliged to comment on the physical aspects of a weighty, unillustrated book headed for desk and library and not for the coffee table, now and then there should be exceptions. This is one instance. Occasionally publishers, by their manifest regard for the appearance of the pages and the strength of the binding of a book seem to be telling potential users that this is a book for the decades to come. The people at the Cambridge University Press have received many tributes for their efforts on these volumes.

More important are judgments concerning what is on the pages and between the covers: did this volume deserve its reputation as a model of collaboration and collation on a difficult topic? First, one does well to listen to the editor's intentions, stated in a typically modest and succinct preface: "We have tried to give . . . an account of the text and versions of the Bible used in the West, of its multiplication in manuscript and print, and its circulation; of attitudes towards its authority and exegesis; and of its place in the life of the western Church. And, with much reserve, something has been said of the impact of the Bible upon the world" (p. ix).

Here one's first occasion for uneasiness and disquiet appears. In January of 1963 that serene British view of the worthwhileness of the topics could be stated without further explanation to legitimate and locate this kind of history. The Vatican Council, then coming to a climax, was exposing to the world the effects of a generation of Biblical recovery and serving notice to Protestants that Catholics also took the ancient Scriptures seriously. Protestantism needed the Bible for its life in those high years of the ecumenical movement, in the later days of neo-orthodox (classic) theology's regnancy and during ongoing Biblical revival.

Less than a decade later, though the editor could not have been expected

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to foresee all the details of the subsequent devastations of that world, little can be taken for granted. Though the Bible remains a best seller and while new versions of it are better than most of their predecessors, the Bible's "place in the life of the western Church" has been called into question. Post-Vatican II Catholicism has seen conservative reaction back from the Bible into other traditions and revolutionary forward movement away from the Scriptures. Protestant radical theology and the presentist cult of relevance have led many to question the validity of any historical approaches, including the Biblical, in the modern world. Professor S. L. Greenslade and his colleagues present their topics without explanation or apology, and there is seldom a hint of the clash and conflict with the world or in the Church that characterizes the place of the Bible today.

If a kind of Anglican serenity deprives the book of some drama, an English provincialism serves to limit its scope. Perhaps we are all parochial, and it is our parochialism that leads us to see the narrowness of Anglican "comprehension" most of all. Thus F. L. Cross's notable *Oxford Dictionary of the Christian Church* (1957) was curiously weighted to make "Christian" often seem to equal "Anglican." True, Greenslade reminds us that selection and proportion led his coworkers to concentrate on the needs of English-speaking readers. But English-speaking readers can work with translated continental works where they cannot read the originals. And America is part of that English-speaking world; yet America is short-shrifted throughout. About a dozen American versions are referred to, and three or four committees or societies for translating or propagating the Bible receive mention, but the place of the Bible in America hardly comes up. This is a curious omission. Historian Jerald C. Brauer's contention that "a sustained effort to avoid going beyond the truth and light already known in the Bible" (as a counterpart to "constant free experimentation") has been the basic motif of Protestant America's Protestantism and has stood up well.[1] Yet this place is in no way observed; not even a giant like Jonathan Edwards, who wrote a widely believed charter for America on the basis of Biblical history, receives a line of notice.

The authors tend to minimize conflict once they move past the religious wars between Protestants and Catholics over the Bible back in the Good Old Days of the sixteenth century. Nowhere is that more evident than in Alan Richardson's very tempered and balanced account of modern Biblical interpretation, especially when he does look across the Channel to modern German disputes. Of the most controversial recent giant, a man much set upon by conservative evangelicals, he feels constrained to say, "[Rudolf] Bultmann is at heart an evangelical preacher" (p. 325). Well, in a way, yes—but he stirred up more troubles and wanted to stir up more than that pacific sentence with its specific connotations sets out to imply.

[1] Jerald C. Brauer, *Protestantism in America: A Narrative History* (Philadelphia, 1965), 7.

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If these criticisms deprive the whole of drama, they help build confidence in the parts of this collection. The authors are not overt special pleaders; they bend over backward to be irenic and fair, and the reader can have a sense of assurance that here is an honest and fair-minded accounting, an impartation of information needed by anyone who wishes to set the Bible in its historical contexts.

We have already taken notice of Greenslade's devotion to continental versions in the earlier period; the forty-six pages devoted to these are truly excellent compilations of material on the sixteenth century. The twenty-one pages devoted to the next three and one-half centuries are more sketchy. The authors do much better with ongoing English versions, and there is a surprising and thoroughly appropriate chapter by Eric Fenn on "The Bible and the Missionary." In the act of translating and promoting the Bible beyond the West, Christians engaged in a kind of intercultural contact whose full story has not yet been told.

F. J. Crehan, S.J. wrote a then-welcomed chapter on post-Tridentine Catholicism's use of the Bible; that it needs more updating than any other chapter is not the fault of the author. From the viewpoint of general historians like myself, as opposed to Biblical specialists, perhaps Norman Sykes's chapter on "The Religion of Protestants" will have widest appeal. Sykes examines in the light of subsequent history William Chillingworth's statement in 1638 that "THE BIBLE, I say, the BIBLE only, is the religion of Protestants," and he shows just how complex support of that assertion would have to be. W. Neil follows that up with an excellent essay on "The Criticism and Theological Use of the Bible, 1700–1950." This chapter is filled with a sense of drama and ends with a quotation from P. T. Forsyth that modifies Chillingworth significantly: "The Gospel, and the Gospel alone, is the religion of Protestants" (p. 293). Neil shows how difficult the living out of that intention has been in a world where scholars squabble over what is the Gospel and how the Bible records it.

Not everyone, including the authors who contributed to this volume, would agree with Gerhard Ebeling's claim that church history is the history of the interpretation of the Bible; that sounds too intellectualistic for many historians' ears. But Greenslade's book goes as far as books for the English-speaking world usually go in substantiating portions of what is implied in such a claim. Within the very broad limits they have set upon their inquiry the authors have succeeded in producing the one book about the Bible that general modern historians ought most to want for their shelves.

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Reviews of Books

GENERAL

HASKELL FAIN. *Between Philosophy and History: The Resurrection of Speculative Philosophy of History Within the Analytic Tradition.* Princeton: Princeton University Press. 1970. Pp. xi, 329. $10.00.

HANS-WALTER HEDINGER. *Subjektivität und Geschichtswissenschaft: Grundzüge einer Historik.* (Historische Forschungen, Number 2.) Berlin: Duncker & Humblot. 1969. Pp. 691. DM 118.

ERWIN HÖLZLE. *Idee und Ideologie: Eine Zeitkritik aus universalhistorischer Sicht.* Bern: Francke Verlag. 1969. Pp. 252. 28.80 fr. S.

The problem of subjectivity in historical knowledge has played a major role in German as well as Anglo-American philosophy of history since the turn of the century and in both language areas has led to serious questioning of the possibility of objectivity in historical writing. From very different perspectives theorists and historians such as Ernst Troeltsch in Germany, Benedetto Croce in Italy, and Carl Becker and Charles Beard in the United States decided long ago that all historical writing reflected the personality, time, and culture of the historian to an extent that made impossible any understanding of the real process of history and that philosophy of history henceforth must not deal with history itself but with the thought processes of the historian.

All three of the works under study represent challenges to these assumptions, and at least the first two constitute serious attempts to return to a sense of reality and rationality in historical inquiry. Each is deeply rooted in the intellectual heritage of its language area—Fain in analytical philosophy, Hedinger and Hölzle in German historicism and idealism—and none fully escapes the limits of its heritage. The Fain and Hedinger books again reflect how isolated the Anglo-American and German discussions have been from each other. There is little indication that Fain is aware of contemporary discussions in philosophy of history on the Continent; Hedinger frankly admits that he has generally not considered the non-German literature (p. 42 n.9). Yet it is interesting to what extent they arrive at similar positions. Fain represents an important revision of Anglo-American neopositivism; Hedinger an even more significant re-examination of German historicism.

Fain begins his book by regretting the alienation of philosophy from history in Anglo-American thought since the eighteenth century. Logical positivism, he observes, has falsely assumed that philosophy and history are conceptually independent of each other, that "the philosopher's task consists solely in examining the logical relations between the propositions of science (and of history)" and that of the philosopher of history in examining the language of the historian (p. 15). This attitude, Fain notes, is deeply rooted in a nineteenth-century positivistic identification of science in general with the hard sciences—for example, physics, which finds its extreme formulation in the Hempelian "covering law" model. This position has led to the denial of history as a legitimate area of scientific study. For Fain philosophy is concerned with the search for criteria of intelligibility and philosophy of history with the search for criteria of historical intelligibility. The search for the latter leads the philosopher and the historian to a confrontation with concrete historical reality. The establishment of facts is not in itself a philosophic act, but the historian always goes beyond this: he

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wishes to construct a story, not any story but a truthful story. Such a story requires conceptual guidelines, which in turn require the speculative assumption that there is coherence in history. Collingwood's insistence that written history is nothing but the re-enactment in the mind of the historian of past thought overlooked that there are developments in history over which men have no conscious control. All historical narrative requires explanations—not on the Hegelian or the Hempelian model, but genetic explanations that seek to understand how one unique historical situation gave birth to another. Just as one can understand the mechanics of a clock without knowing the laws of mechanics, Fain argues, so one can understand some of the mechanics of social, political, and intellectual change without knowing the laws of history.

Within the analytical tradition Fain still views philosophy of history as essentially a cognitive activity relatively isolated from practical activity. Hedinger, much more aware of the problems raised by contemporary continental philosophical thought, views cognition as an expression of an existential life situation. "Human life," he observes, "does not take place in a world which is objectified in the sense of the natural sciences but in a life world" (p. 604). Yet Hedinger wants to overcome the radical subjectivism and anti-intellectualism inherent in the identification of subject and object, of theory and practice. Hedinger's book, originally a doctoral dissertation, is perhaps the most ambitious attempt in Germany since Droysen and Dilthey to write a logic of historical inquiry and to offer an answer to the question of how, given the subjectivity of all knowledge, a science of history is possible. It possesses, however, much greater critical distance from the German tradition of historical thought than does Hans-Georg Gadamer's much discussed *Wahrheit und Methode* (1960). Hedinger questions the classical German historicist distinction between the natural and the human sciences. History as a science, he insists, has the same aim as other sciences, namely to formulate binding, intersubjectively comprehensible empirical sentences (*Aussagen*) that are true, have general validity, and can be tested intersubjectively as to their truth value. All cognition is related to a concrete historical situation, but this *Standortgebundenheit* of all knowledge does not negate its truth value. The object of the historian's study itself has a structure that imposes itself upon the historian. To be sure, the historian always sees his historical subject matter from the perspective of his situation, but perspectivity does not mean arbitrary subjectivity; indeed "different perspectives can supplement but will not contradict each other" (p. 631). It is thus possible, within limits, for the historian not only to formulate generalizations and laws of limited scope but to fulfill the Rankean demand to reconstruct the past "wie es eigentlich gewesen" (p. 663).

Hölzle's book is very different in tone and style from the preceding two works. It is not concerned with the theory of knowledge in a narrow sense. It is, rather, an attempt by a senior German historian who throughout his life has been deeply committed to traditional German historical concepts and values to come to terms with the changed intellectual and political situation throughout the world since 1945. Hölzle has read extensively in the contemporary sociological and historical literature, German and non-German (especially American), Marxist and non-Marxist, and seeks to overcome the separation of political and intellectual history from social history traditionally maintained by German historians until relatively recent years. Yet the examination of this extensive literature leads Hölzle back to the traditional German idealistic conception that history is the history of thought and that the crucial decisions in history are made by powerful statesmen motivated by these thoughts. This seems to be the point of the lengthy analysis of changing conceptions of ideology since the French Revolution that constitutes the central part of the book. Hölzle rejects the identification of ideology with false consciousness and argues that all cognition contains an element of truth that transcends the subjectivity and social relatedness of the observer. The end of ideology so often proclaimed since 1945 has not occurred; rather, ideologies and powerful leaders play a more important role in the twentieth century, especially in the non-European world, than they did in the nineteenth. Ideas, never clearly distinguished from ideologies, thus dominate the world, and the history of ideas and of ideologies therefore becomes

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crucial for an understanding of the modern world. To be sure, ideas must not be understood as abstract concepts but rather, as Ranke viewed them, as forces operating in history and immersed in reality. "The history of ideas," Hölzle notes, "is a part of history as a science of reality" (p. 233). Yet despite Hölzle's interest in social and structural history, the relation of ideas to social reality never becomes clear. If I do not misunderstand Hölzle, history must include social history and broaden its scope to include the non-Western world, but the determining factors in historical change remain ideas and political actions that always occur within concrete social and historical contexts but can never be understood in terms of them. In the final analysis, for Hölzle as for Ranke and the German *Verstehen* tradition, history remains an unfathomable process that requires the sympathetic and empathetic understanding of great men and that defies rational analysis.

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Historische Forschungen in der DDR, 1960–1970: Analysen und Berichte. Zum XIII. Internationalen Historikerkongress in Moskau 1970. (Zeitschrift für Geschichtswissenschaft, Volume 18, 1970, Sonderband.) Berlin: VEB Deutscher Verlag der Wissenschaften. 1970. Pp. 836. DM 28.

This volume continues the decennial reports on historical research in the German Democratic Republic initiated by a similar publication on the occasion of the meeting of the International Congress of Historians in Stockholm in 1960. As was the earlier volume, this one is presented under the auspices of the *Zeitschrift für Geschichtswissenschaft.* Forty-one bibliographical essays provide what the foreword calls a reckoning of the achievement of Marxist-Leninist historiography in the 1960s and, by indicating lacunae, an encouragement for further research. The essays may be grouped in order as follows: two on historiography; eleven on topics such as agriculture and economy; one on ancient history; thirteen on German history from earliest times to 1949 by period; three on German history since 1949 by political division; three on Russia and its relations with Germany; one each on European socialist countries, European capitalist countries, and the United States, Africa, Latin America, Asia, reference works, libraries, and archives. In most cases the essay begins with a description of the issues vital to the topic, often as defined by Lenin or Walter Ulbricht, and concludes with a brief reference to the tasks ahead. Essays range in length from six pages on libraries to thirty-eight pages each for research on Marx and Engels and on the Third Reich. More than a third of the space is devoted to footnotes. A list of the authors and their positions and another of abbreviations used completes the volume. There is no index.

A book as broad and diverse in content defies comment on each essay, but taken together the essays indicate aspects of the organization, direction, and problems of research. First, the book reflects decisions made over the past two decades by political leadership. Beginning in 1951, as the initial essay points out, the Central Committee of the Socialist Unity party made decisions relating to such issues as the publication of documents, the founding of historical institutes, the creation of a historical museum, and the selection of topics for special emphasis. In 1955 the focus was placed on basic problems of German history and the German labor movement, in 1963 on the general laws of socialist development, and in 1967 on creating socialist life styles and thought processes. The organization of this volume, the titles of dissertations and *Habilitationschriften* it contains, and the listing of multivolume handbooks on German history and the labor movement testify to the effectiveness of official orientation. Second, as a result of this orientation, the German past as seen between the Elbe and the Oder is the history of the working class as the agent of an inexorable movement leading to the creation of a socialist state. The workers, therefore, have been the ones pursuing the best interests of the German nation over against the feudal-bourgeois, antinational defenders of class interests. Third, in defense of the national interest, East German historians have concentrated especially on exposing the conclusions of West German research as fallacious. Fourth, certain topics are either slighted or ignored. The Peoples Republic of China goes unmentioned in the essays on the socialist world system and on Asia, as does Albania. The history of the youth

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movement omits both the Wandervögel and the Hitler-jugend, while the history of higher education ignores students. Neither Jews nor anti-Semitism nor the arts are significant parts of the East German past. Relations of Germany with the non-Russian world are all but forgotten. And the *Bauern* get short shrift in the *Arbeiter-und-Bauern-Staat*. Fifth, historiographical controversy is rare in East Germany. Only the dating of the Industrial Revolution, the decline of liberalism and its relation to democracy, the nature of ancient societies, and the creation of the medieval empire appear to have aroused argument. Criticism is even more rare; only three individuals' work and a *Festschrift* celebrating the history of the medical faculty at Leipzig were found wanting.

Although these essays reveal the shape of historical research, they are more reportorial and less analytical than the title indicates. The essays on Latin America and higher education are the closest to being analyses and the one on lower schools the best written, but most are scarcely more than the repetition or close paraphrasing of titles readily found in the footnotes. The more important of these titles are discussed, not always with identical bibliographical data, in several essays. This repetition, coupled with the near absence of cross-references to other essays, raises questions about the effectiveness of editor-collectives. Indeed, the information this book contains would have been more expeditiously conveyed in bibliographical form with modest annotations where necessary.

Useful for historians of Germany, especially those whose research may take them to the Democratic Republic whose repositories and historical institutes' specialties are briefly discussed, this book is a tribute to a prodigious and sustained effort to shape future thought processes and life styles by creating an appropriate model of the past.

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ROBERT E. BROWN. *Carl Becker on History and the American Revolution*. East Lansing, Mich.: Spartan Press. 1970. Pp. vi, 285. $7.50.

JOHN HIGHAM. *Writing American History: Essays on Modern Scholarship*. Bloomington: Indiana University Press. 1970. Pp. x, 207. $6.50.

DAVID HACKETT FISCHER. *Historians' Fallacies: Toward a Logic of Historical Thought*. New York: Harper and Row. 1970. Pp. xxii, 338. $10.00.

These three books may be taken in ascending order of generality. In several previous works Robert E. Brown has upheld the view that society in the American colonies was "democratic" long before the break with the mother country. Brown has also become known as a harsh critic of other historians whose theories and scholarship fail to meet his standards of correctness. Charles A. Beard's *An Economic Interpretation of the Constitution* was among his first targets. He now turns his attention to Carl Becker. As his title implies, Brown is concerned in part to test Becker's treatment of the era of the American Revolution, and in part to scrutinize Becker's wider speculations as to the nature and purpose of history itself. On both counts he gives Becker low marks, for erudition and analytical power and also for honesty and elementary common sense. Brown maintains that the famous Becker thesis about "home rule," i.e. class conflict within pre-Revolutionary America, can be disproved from the very sources that Becker cited. Brown points, too, to the inconsistency in Becker's various statements: sometimes the inequality of eighteenth-century America is stressed, and sometimes the equality of condition—at least, relative to that of Europe. Much of what Brown says in this respect is sound. Becker's reputation has slumped—witness the critique of his *Heavenly City* at the hands of Peter Gay. But Brown's analysis seems sour and at times obtuse. His contention is that Becker deliberately falsified the evidence because he had become a "subjective relativist" for whom historical truth was subordinate to the social needs of his own day. Becker's whimsicalities are taken as disingenuous evasions. He is portrayed as having lost faith in the common man, as advocating something Brown with every indication of contempt labels "collective democracy"—which leads Becker to skate over "the thin ice of Communism." One reason for this exaggerated insistence on Becker's radicalism, presumably, is that Brown has to account for the resurgence in recent years of class-conflict interpretations. The Beckerian heresy has broken out again; evil is all about us. We cannot gather from

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Brown's accusatory chapters why Becker was so much admired by his fellows, unless we are to take them as cretinous dupes. Perhaps he was over-valued because he wrote with such assured felicity. His changes of direction do indicate an infirmity of purpose, as Cushing Strout suggested some years ago. There is a case against Becker. Brown has spoiled his case by making it an indictment. And those who attack others for slovenliness should double-check their proofs. One's confidence is hardly bolstered by a book that refers to Malcolm Cowley as "Crowley," or throughout misspells "consensus."

John Higham's essays, most of them reprinted but with considerable revision, are altogether more distinguished. He too can be a severe critic. But in his dry, understated way he is preoccupied with building up, not with knocking down. Like many contemporary historians he is fascinated by historianship. This book, he says, may be taken as a supplement to *History* (1965), the volume he published in collaboration with Leonard Krieger and Felix Gilbert. It ranges over two main sets of problems: the nature of intellectual history, and the broader alternations of American historiography in the twentieth century, which Higham groups under the familiar rubric of "Conflict and Consensus." He is much more aware than Brown of the complexities of the historian's task and more generous to the Turner-Beard-Becker triumvirate. He knows that when we are dead we all have feet of clay, literally as well as metaphorically. Yet he is not lugubrious in consequence. He sees that the relativism inherent in the notion of successive "climates of opinion" (the phrase Becker borrowed from Alfred North Whitehead) need not paralyze us. If each generation has a distinctive pattern of ideas and appetites, then it ought to be possible for us to recognize bygone patterns and elucidate them. This is the task of the intellectual historian. For Higham it follows that the richest form of intellectual history is that which spans an era, embracing everything from popular culture to high culture. As a sample he offers a penetrating essay on American culture in the 1890s, weaving together various instances of a new passion for physical and mental activity. "Weaving" is an appropriate word: Higham perceives unifying features of which people in the 1890s may have been unaware and convinces us that the pattern was actually there. His meditations on comparative history enable him, moreover, to avoid the narrowness of one-nation history, in which phenomena international in scope are too often explained hermetically. In the most complimentary sense, John Higham is a historian's historian.

David Fischer is a historian who would like to shake the profession until its back teeth rattle. Exasperated by the conceptual fuzziness and the meagerness of spirit of his peers, he sets out to define a logic of historical thought that is to improve the practice of history writing. He admits that the task is difficult. He believes, however, that an essential preliminary is to clear away the undergrowth; and so he addresses himself mainly to historians' fallacies: bad logic, bad writing, and occasional bad faith. The result is an exuberantly iconoclastic catalog of errors, according to categories adapted chiefly from logicians. He is surely right in his contention that most of us do not think clearly enough and that we are intellectually sluggish. We have done those things that we ought not to have done. We have left undone those things that we ought to have done. By the time we have got to Fischer's last chapter we may be ready to conclude that there is no health in us. The list of fallacies, including even the "fallacist's fallacy" (p. 305), appears endless. With a mixture of guilt and *Schadenfreude,* following Fischer's often witty and acute exposés, we are instructed that the sinners number such great names as Taine, Michelet, Tocqueville, Carlyle, Marc Bloch, and Georges Lefebvre among the Europeans, and Charles A. Beard, Daniel Boorstin, Carl Bridenbaugh, Oscar Handlin, Richard Hofstadter, Samuel Eliot Morison, David Potter, Arthur M. Schlesinger, Jr., and C. Vann Woodward among twentieth-century Americans. But ultimately the book is a miscellany rather than a sustained analysis. One's doubts multiply instead of being resolved. The example he offers of a fallacy (the remarks on A. J. P. Taylor, pp. 73–74, are a case in point) does not always fit. His accusations (e.g. against Boorstin, p. 57) are apt to be too brief to carry conviction. The roll of categories, initially amusing, becomes bewildering: Occam's Razor was needed. Some of the fallacies proposed are not fallacies at all but literary devices—perfectly defensible in

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themselves. On the whole Fischer writes well. But in a book of this nature his own slips stand out (e.g. "the latter" to identify the last of more than two items; Edward instead of Edwin Arlington Robinson on page 42 just after he has scolded historical relativists for encouraging factual sloppiness). And where error seems so nearly universal, ought not Fischer to have considered a different hypothesis from his principal, Menckenian assumption that historians are numskulls? History, he says, is "not story-telling but problem-solving." Well, yes, if we want to go beyond costume-drama. More rigor, by all means. But the problem-solving as distinct from problem-posing record of even the more avowedly rigorous social sciences is not dazzling. As historians we can do better, and ought to. Relativism has gone too far. But ought we not to face a fundamental problem, with which Becker and other relativists did try to grapple: namely, that history in common with most other branches of learning never does, never can, reach ultimate, definitive "truth" on the major questions of human behavior?

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P. M. HOLT *et al.*, editors. *The Cambridge History of Islam.* Volume 1, *The Central Islamic Lands;* Volume 2, *The Further Islamic Lands, Islamic Society and Civilization.* New York: Cambridge University Press. 1970. Pp. xviii, 815; xxvi, 966. $19.50 each, $37.50 the set.

VOLUME 1: The Cambridge phalanxes march on. This first of two volumes retells the history of the central Islamic lands—the Arab east, Persia, Turkey, Russia, and Central Asia. Volume 2 deals with the further Islamic lands—the Indian subcontinent, Southeast Asia, and Africa and the Muslim west—and then devotes over 440 pages to Islamic society and civilization. The thirty-two plates are all included in volume 2.

The editors, who hail from the high command of British Orientalism, have contributed chapters to volume 1 and have enlisted in their ranks eminent scholars from Britain, the Middle East, France, Germany, Italy, Canada, and the United States. The work is designed to appeal to "the expert orientalist," to "students in other fields of history," and to "those who read history for intellectual pleasure" (p. ix). Of these three categories only the first is likely to derive full benefit. The price is beyond the reach of most students. The narratives are heavily freighted with details unfamiliar to all but the experts. The index runs to about sixty pages; of the forty-five entries under "Z" only three or four are apt to be known to the layman. The glossary is much too short. The dynasty lists are limited to seven without regnal dates, some of which are also not found in the narratives. The mediocre sketch maps are not properly coordinated with the text: Zafar in the text is Dhufar on one map, and the correct form Khokand in the text is Kokhand on two maps (not to be confused with the nearby Khojand, which is in the text but, like many other names, not on the maps).

For the expert the work, these blemishes aside, is a delight. One is grateful to the press and the editors for marshaling the writings of a number of men and three women who present the latest or nearly the latest findings in their respective fields. Islamic history is so variegated that any authority on one or several segments can still learn much from this broad symposium.

Despite the overall excellence of the work, my own delight is not unmitigated. At times the marching scholars fall out of step. Nearly a century of Egyptian history is skipped between chapters by Holt and Bernard Lewis. The inclusion of separate sections on Turkish, Arab, and Persian nationalisms produces considerable repetition, as do the concluding chapters on communism, the political impact of the West, and economic and social change. Islam as a religion is largely relegated to the second half of volume 2. The *Cambridge History* actually complements the second volume, subtitled *Islam,* of *Religion in the Middle East,* published under the general editorship of the late A. J. Arberry by the same press in 1969, but the history does not point this out.

Selectivity presents a difficult problem in editing a study concerned with three continents and over thirteen centuries, but I should have traded repetitive passages for more space on Shamil of the Caucasus, the Basmachis of Central Asia, or the Assassins of Persia and Syria. Lewis, one of the editors of volume 1, has a whole book on the Assassins; why, then, are they passed over so lightly here? The

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Bektashis get a single mention, but no reference is made to their Shiite complexion or their strength in modern Albania. The Arabian Peninsula receives short shrift once the seat of the caliphate moves away from Medina. The revolt of the Kharijites in 743 is noticed but not their capture of Mecca in 745. The pilfering of the Black Stone by the Carmathians in 930 is recorded, but the significance of the stone is not explained and nothing is said about its return to the Ka'bah. The Sharifs of Mecca show up ca. 1261, but silence prevails regarding their earlier accession to power and their role between the thirteenth century and the twentieth. The Ibadites, spiritual descendants of the Kharijites, are overlooked, though they have elected imams in Oman in recent times. The Arabian Ikhwan of this century with their remarkable similarity to the tribal forces of early Islam do not make these pages. The abortive Anglo-Ottoman attempt in 1913–14 to divide all Arabia into two spheres of influence is likewise missing.

The sections on modern history are the weakest. Scattered references to the 1960s occur, but the terminal dates for the various narratives fall roughly around 1950. I regret that the authors have not given their interpretations of developments during the last two decades, even though I question some of their interpretations for the decades just before 1950. The treatment of the rise and importance of the oil industry in the Middle East is fragmentary and inadequate. The essay on Arab nationalism slights the upsurge of anti-Ottoman sentiment in the Arabian Peninsula from the eighteenth century on. And it is disappointing to find cropping up once again the absurd equation "modernizing (i.e. westernizing)" (p. 364). The history itself shows that the Islamic world has much inner strength to draw upon apart from its borrowing from the West.

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VOLUME 2: *The Cambridge History of Islam*, volume 2, is in reality two works; the first deals with the history of the further Islamic lands of India, Southeast Asia, North Africa, Spain, Sicily, and Africa, while the second section isolates and treats analytically the major aspects of Islamic society and civilization. Editors of general area or even country histories must make a number of crucial decisions. At what intellectual level should the history be written? Should the work be done by one man or even a small group of scholars or by a large number of specialists? The larger the group the less unified the book will be, no matter how diligently the editors may strive, but the more expert will be the individual chapters. Should the work break new ground and be interpretive, even controversial? Or should it summarize the state of the field, presenting conclusions shared by a consensus of scholars and eschewing the novel? The editors' answers have inevitably shaped this volume. The intellectual level is consistently high throughout. Although the editors hope that the book "will not be despised by the expert orientalist," it is meant for "students in other fields of history and particularly university students of oriental subjects [as well as] those who read history for intellectual pleasure." They have correctly identified the group most likely to read this book: university undergraduate and graduate students interested in Islamic history. I myself doubt that many people who read history for intellectual pleasure will turn to a work of close to nine hundred pages written by twenty-seven different authors. The geographical coverage is staggering when one considers that separate chapters deal with Islam in West Africa and Indonesia. In addition the writing lacks pace; it is informative and expository but far from distinguished. No doubt the people who read the volume will be students and scholars who want a clear and concise summary of the history of an area or an introduction to the major characteristics of Islamic civilization. Their further reading will be facilitated by a short but useful bibliography at the end of the volume.

A second question the editors faced concerned the number of contributors. They had probably little choice but to select a large number because of the wide geographical area being covered. Few scholars are able to generalize about Islam in such a diverse geographical setting. Accordingly the editors selected experts to write about their specialties, a solution that created problems of integration,

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not always satisfactorily solved. Regional differences and similarities within Islam raise questions that greatly interest many scholars. A growing number of Africanists, for example, are trying to understand how Islam altered African societies and how it was in turn altered; they will be disappointed by this volume. The area histories are self-contained and do not examine comparative questions about Islam. Part 2 presumably does, for here contributors consider the basic institutions and aspects of Islamic society. There are chapters on the sources of Islamic civilization, Islamic economy, society, and institutions, law and justice, religion and culture, mysticism, and revival and reform. Appearing as they do in a volume devoted to the further Islamic lands, they should take account of Islamic variations and similarities. Unfortunately they do not. The authors writing these chapters are experts on classical Islam in the Arab and Turkish heartland; their exposition derives from this knowledge.

Composite books of this kind generally seek to be compendia of knowledge and to summarize the state of the field. *The Cambridge History of Islam,* volume 2, is no exception; it tells us where we are today and what conclusions have emerged and command some consensus. Although many of the individual authors are the most imaginative historians of Islam—Gustave von Grunebaum and Claude Cahen to mention but a few—they have restrained themselves here. Scholars will go to their other writings for more controversial and stimulating hypotheses. Another clear commitment is toward comprehensiveness. Although the editors claim that *The Cambridge History of Islam* "is not a repository of facts, names, and dates and is not intended primarily for reference, but as a book for continuous reading," certain chapters cannot escape such a use. Humphrey Fisher's chapter on Islam in Africa, for instance, covers so much territory and time—Islam in West and East Africa from earliest times to the present—that its chief value must be a cataloging of names and dates.

Worthy of special comment is X. de Planhol's essay on the geographical setting of Islam in which the author explores the question of Islam's geographical concentration in arid areas. In the conquest of the Muslim heartland nomadic peoples played a dominant role using their knowledge of desert conditions. Islam became pre-eminently a civilization of town dwellers and Bedouins as it had been at its birth in Arabia. The peasantry played a much less effective role, and their prestige remained lower than that of Bedouins and town dwellers. The spread of Islam to its outermost limits, Indonesia and tropical Africa, on the other hand, was mainly effected by merchants and scholars rather than armies and Bedouins. De Planhol's essay is valuable and rather unusual in this volume particularly because it tries to encompass Islam in its total geographical environment and points out major variations. A similarly spirited essay, Fazlur Rahman's "Revival and Reform in Islam," must be commended for its effort to grapple with continuities and discontinuities in Muslim reformist movements. Early revivals—like Wahhabism, the Sanusis, and the Fulani jihad—are seen as movements against corruption, lax practices, and in some cases Sufism within Islam. Later reformism, the author contends, drew upon this tradition and was not merely a function of the impact of the West.

Although this volume will occupy a regular place on the library shelves of scholars of the Muslim world, it cannot do all that the editors claim. It will be used as a reference work. It will not be a book for continuous reading; its size and lack of analytical focus preclude its replacing the best known introductory essays on Islam, such as H. A. R. Gibb's compact and more vigorously argued *Mohammedanism,* first published in 1949.

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CHARLES VERLINDEN. *The Beginnings of Modern Colonization: Eleven Essays with an Introduction.* Translated by YVONNE FRECCERO. Ithaca: Cornell University Press. 1970. Pp. xxi, 248. $9.75.

The author has assembled in this book in English translation a number of his articles on the transition from medieval colonization to the colonization of modern times. Readers familiar with his work will not be surprised at the emphasis he places on the part played by Italians in this process. He argues that it is

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customary to think of colonial history in the strict or narrow sense as beginning immediately after the great discoveries. Sometimes allusion is made to medieval precedents but, as a rule, either very briefly or superficially. One of his aims is to stress that the concept of continuity between the Middle Ages and modern times can be applied to colonial history in the strict sense. It is his contention that the Italians in general and the Genoese in particular acted as agents of transition from medieval colonization to the Iberian varieties.

The essays are grouped in three parts, respectively entitled "Europe and America," "Early Italian Colonization," and "Early Western European Colonization." For my money, the second part is the most interesting and the most convincing, together with the final article on "Feudal and Demesnial Forms of Portuguese Colonization in the Atlantic Zone in the Fourteenth and Fifteenth Centuries, especially under Henry the Navigator." Verlinden shows how medieval colonization in the Levant was primarily the work of the Italian city republics, which established colonies—and not merely commercial outposts as is often assumed—first in the Holy Land and then on the remains of the Byzantine Empire along the shores of the Aegean, the Ionian, and even the Black Seas. These colonies used a slave economy like that developed subsequently on a greater breadth and a vaster scale in the colonies founded in America by Spaniards, Portuguese, Dutch, French, and English. Cyprus and Crete, with their sugar plantations, were especially important in this respect. The author gives us a fascinating analysis of some contracts for the sale of slaves. They certainly bear out his contention that in this aspect, as in many others, the medieval Mediterranean colonial economy had characteristics that prefigure the colonial economy of modern times in the Atlantic region.

As regards the Negro slave trade, Verlinden reminds us that in the high Middle Ages numerous Sudanese and Guinean slaves were brought to the African shore of the Mediterranean by trans-Saharan caravans and then sold to Christian merchants who marketed them in eastern Spain, southern France, and Italy. During the second half of the fifteenth century, the Portuguese re-routed a great part of this trade, as they re-routed much of the trans-Sahara gold trade at the same time. In both instances, from an overland trade with Muslim and Italian intermediaries, they developed a direct maritime trade with West Africa for gold and slaves, exactly as they did in the following century with the spices from the East Indies.

In an interesting chapter on "The Italian Colony of Lisbon and the Development of Portuguese Metropolitan and Colonial Economy," Verlinden briefly describes the varying roles of Florentines, Lombards, Venetians, and Genoese, some of whom, together with others like Luca Giraldi, have been further studied by Virginia Rau and V. Magalhães Godinho. Similarly, the author's chapter on "Italian Influence on Spanish Economy and Colonization during the Reign of Ferdinand the Catholic" is a reprint of a pioneer article of 1954, the scope and depth of which have been greatly extended in recent years by the work of one of Verlinden's former students, Ruth Pike, in her *Enterprise and Adventure: The Genoese in Seville and the Opening of the New World* (1966). But even if recent researchers have widened and extended some of the paths first blazed by Verlinden, it is convenient to have these seminal essays reprinted in a translation that reads smoothly enough. This book is a "must" for all those interested in the origins and techniques of early European expansion overseas and the shift from a Mediterranean to an Atlantic maritime economy.

C. R. BOXER
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JIM ALLEE HART. *Views on the News: The Developing Editorial Syndrome, 1500–1800.* Foreword by HOWARD RUSK LONG. (New Horizons in Journalism.) Carbondale: Southern Illinois University Press. 1970. Pp. x, 238. $7.95.

THEODORE P. GREENE. *America's Heroes: The Changing Models of Success in American Magazines.* New York: Oxford University Press. 1970. Pp. vi, 387. $9.50.

SIDNEY KOBRE. *Development of American Journalism.* (Journalism Series.) Dubuque: Wm. C. Brown Company. 1969. Pp. xiv, 767. $8.75.

All three books deal with different aspects of the history of journalism. There the resemblance largely ceases. *Views on the News* is a com-

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prehensive but relatively compact history of the English-language newspaper editorial from its beginnings in England in the first half of the sixteenth century until its emergence as a distinct part of the American newspaper about 1800. Theodore P. Greene's *American Heroes* makes use of the history of American magazines in four different periods to establish the changing models of success in the minds of magazine editors and their readers. In *The Development of American Journalism* Sidney Kobre provides a somewhat diffuse textbook treatment of the history of American newspapers analogous to the surveys by the late Frank Luther Mott, Edwin Emery and Henry Ladd Smith, James M. Lee, and others. The Hart and Kobre volumes overlap in a number of places, but the subject matter of Professor Greene's stimulating study is independent of both.

The author of *Views on the News* is no newcomer to the field of the history of American journalism. An associate professor of journalism at Southern Illinois University, he is also the author of a competent history of the St. Louis *Globe-Democrat*. Although the emphasis of his study is predominantly American, he gives much attention to the evolution of opinion making in the British press, culminating by 1800 in the leading article or "leader." Withal the author makes clear the significance of wars and periods of active political controversy on editorializing in both England and America.

Professor Hart has interesting things to say about the influence of pamphlets on opinion making before it became a newspaper function. Among the distinctive features of the pamphlet were that it was not written primarily for commercial gain and that it was much more lengthy than the modern editorial. In America the pamphlet continued to be used by opinion leaders for the dissemination of their ideas long after its vogue in England had passed. By 1800, however, the American pamphleteer was fading rapidly into the background, and newspaper editors like Noah Webster and Benjamin Russell were emerging as the chief opinion makers.

Although Hart makes clear that the term "editorial" was originally an adjectival modifier of the word "article" and referred to an article written by the newspaper editor, nowhere in this book does he discuss the origin of the word "editor." Also the repeated references in the text to the importance of the freedom of the press as a favorite theme of editorial paragraphs would seem to have justified an appropriate entry in the author's otherwise satisfactory index.

Greene, a professor of American studies at Amherst College, explains in his introduction that his book originated in his desire to discover what has happened to individualism in America. Making use of selected magazine files and of magazine biographies in particular, Greene examines, somewhat as Dixon Wecter did before him, the transformation of the concept of the American hero from the neoclassic model of the magazine world of 1787–1820 into the various other models that emerged by the end of World War I. The final stage, as he sees it, was the new "organization-man" bureaucratic hero of the 1920s.

Undoubtedly Greene's findings illuminate a significant area of American intellectual history as well as the history of American magazines. This book points up the contrast between the emphasis late eighteenth-century magazine biographers placed on the intellect and character of their subjects and the different emphases of their counterparts during the magazine revolution of the 1890s, the Progressive era, and the First World War. Power and personal fame were preoccupations of the successful captains of industry, artists, and other individuals portrayed in magazine articles of the 1890s. In the strenuous Progressive decade that followed magazine biographers for the first time dealt extensively with villains as well as with heroes. The Progressive model of success developed in these biographies was a mixture of old and new elements. The chief new element was the insistence on social contribution—social service—as a standard for the preferred life style. Also, according to Greene, during the Progressive era magazine biographers subordinated mental traits to physical attributes and other human interest qualities to a greater degree than in the 1890s. The diminishing importance of assertive individualism in the period after 1913 was reflected in the reduced number of magazine biographies and the focus of maga-

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zine writers on the acceptance and expectations of others. In his epilogue Greene looks ahead to the reaction of alarm to the ideal of the organization man that materialized later.

In a concluding note on procedures the author explains his sampling techniques, conceding that they may not seem rigorous enough for specialists in content analysis. He expresses skepticism, moreover, of the use of quantitative methods of research to avoid subjectivity, although he makes discriminating use of statistics.

The first half of Professor's Kobre's *Development of American Journalism* is largely an elaboration of his *Foundations of American Journalism* (1958). Here Kobre follows the practice of his earlier volume of interpolating material on the social, economic, and political background of the various stages of press development. Probably the most useful part of his book is the concluding section on chain and syndicate journalism since 1900, to which about one-fourth of the space in the book is allotted.

In places the style of the Kobre volume tends to be encyclopedic, and there are rather too many errors of fact and usage. The statement that the U.S. Supreme Court ruled that Dred Scott "was not even a citizen" is too sweeping. It is also untrue that "Southern papers were not wealthy enough to have correspondents in the battlefields" of the Civil War. Some characterization other than "vigorous journalism"—perhaps "ring-dominated"—would seem to be more applicable to the Philadelphia newspapers of the Gilded Age. Kobre's explanation of the origins of the Spanish-American War, moreover, fails to take into account the revisionist findings of Ernest R. May and other recent writers.

In summary this most recent history of American journalism is a useful reference work for historians and librarians, but it is somewhat less satisfactory in interpretation and content that Mott's *American Journalism, A History* (1962) and Emery and Smith's *The Press and America* (1954).

J. CUTLER ANDREWS
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EDGAR ANDERSON. *Senie kurzemnieki Amerikā un Tobāgo kolonizacija* [The Ancient Couronians in America and the Colonization of Tobago]. Daugava: Daugavas Apgāda. 1970. Pp. 371.

Under two capable and interested leaders, Jacob and Friedrich Casimir (1638–98), the Duchy of Courland played a small but somewhat dramatic part in the colonial struggle in the seventeenth century. Africa, Brazil, and Norway were objects of effort, but the island of Tobago in the West Indies was for a longer time and with greater zeal the prize the dukes sought to colonize and develop. For another ninety years Courland sporadically showed an interest in the island, and in the last few years a cultural movement has revived a curiosity about it among Latvians in this country and at home.

Edgar Anderson has in his study ranged far for material and has put together, practically, what can be rescued from the past about the Courlanders in the area. In a labor of love he has made these facts into a narrative that should be translated into English, because it illustrates so well the turmoil—diplomatic, naval, and economic—that was part and parcel of the West Indian scene in the seventeenth century. Courland sent over thirty expeditions to the area; nineteen of them got there. The home conditions that can in part be blamed for the failure are well detailed in an English summary; but even more can be blamed on the ambitions of greater powers in whose plans a Courland colony found no place.

Kings and lord protectors of England, kings and admirals of France, and the Heeren Hogen Mogenden and the West India Company of Holland did not recognize the Couronian claims. They had to fight against the Riches, earls of Warwick, who wished to own Tobago; against the Dutch Lampsins, striving to establish a barony there; against buccaneers from L'Ollonois to Sir Henry Morgan; and in the struggle the Courlanders went to the wall. The story is an important one for the Latvian people and for those interested in the colonial period of the West Indies.

The volume has seven maps and fifty-six illustrations; there is no index, but an inclusive bibliography. It is well printed and finely bound, but my copy had twelve blank pages between page 233 and page 256, causing me to guess furiously at the omitted content. For style and content enough can scarcely be said in praise; the one criticism could be that the dichotomy in Courland between the rural,

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servile Latvians and the Germans, dominant in the urban centers, could well be given more stress.

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PHILIP S. BAGWELL and G. E. MINGAY. *Britain and America 1850–1939: A Study of Economic Change*. New York: Praeger Publishers. 1970. Pp. xiv, 312. $7.50.

I am baffled by the presumed "level" of this book. It is "heavier" than a freshman survey text, yet "lighter" than an advanced text, and it is not a monograph. In fact the book seems to be a once-over-lightly survey of each economy in an utterly conventional framework. I am not even certain what a reader who knew nothing of either country's history could learn here. There is no central thesis. It is assumed that the reader knows "the rest," while Bagwell and Mingay bring him up to date on new research in each country's economic history over this stretch of time. If the reader has a modicum of expertise, he will find little that is new here, except that the quantitative material is well organized and presented, which may be worthwhile.

J. R. T. HUGHES
Northwestern University

R. LAURENCE MOORE. *European Socialists and the American Promised Land*. New York: Oxford University Press. 1970. Pp. xxiii, 257. $7.50.

The focus of R. Laurence Moore here is on European Marxist and revisionist views of the United States in the period of the Second International. Having inspected an impressive array of European socialist books, periodicals, and correspondence, Professor Moore concludes that the people he considers—from Marx, Engels, and Wilhelm Liebknecht through H. G. Wells, Lenin, and Trotsky—had less insight into America than Tocqueville, Bryce, and Louis Hartz. He finds, furthermore, that the evolutionary revisionists had a surer grasp of American realities than the cataclysmic Marxists. They took account of the American genius for peaceful, gradual change while the Marxists were always hoping for the worst and were always disappointed. For example, "The brevity of the depression [of 1907] really offered much more objective support to the revisionist contention that the capitalists had learned to moderate the severity of economic crises . . . but, in the initial euphoria of seeing American business floundering, Marxists gave their imagination free rein" (p. 98). The unfortunate Marxists in Professor Moore's account "looking at America wrote themselves into a deep intellectual hole, and none of them seemed to know how to get out. Their opinions on our earlier era are very easy to take apart, and it has not been the primary purpose of this essay to castigate Marxists for astonishing naïveté in order to indulge in fierce anti-Marxist bias" (p. 193). Despite this disclaimer the book suggests that Professor Moore has been, in his wide reading in European socialist documents, an avid collector of Marxist underestimations of the vigor and life expectancy of American capitalism. These trophies of his research he presents with a generous garnish of irony. His Marxists seem perpetually tossed on an emotional pendulum between "unqualified cheerfulness" over what they hope is happening and "chagrin" over the actual speed and direction of American events. Readers who believe that the American trend in the twentieth century is toward a gradual, consensual greening of the society will approve the irony. Readers who sense that the trend is toward convulsion and catastrophe will wonder whether the ironist who scores off prophets of an American socialist revolution might regret his victories.

DAVID HERRESHOFF
Wayne State University

HANS-JÜRGEN SCHRÖDER. *Deutschland und die Vereinigten Staaten 1933–1939: Wirtschaft und Politik in der Entwicklung des deutsch-amerikanischen Gegensatzes*. (Veröffentlichungen des Instituts für Europäische Geschichte Mainz, Number 59.) Wiesbaden: Franz Steiner GmbH. 1970. Pp. 338. DM 44.

The literature of the growing economic and political antagonism between the New Deal and the Nazi regime is enormous. The subject was intensively and competently covered in newspapers and periodicals at the time, and the relevant official papers on both sides have been published in the postwar years in *Foreign Relations of the United States* and *Documents*

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on German Foreign Policy. The archives are open, and Dr. Schröder's excellent bibliography accounts for a large body of analytical commentary. One can safely say that in this area the people's right to know has been well protected.

Ample material has been available, but Schröder argues that previous interpretations have been either crudely Marxian or narrowly conceived in terms of international power politics. In his view it is more revealing to examine the interdependence of domestic and foreign policy in each country. For the early years he finds that Roosevelt and Hitler were struggling so mightily to pull out of the economic depression that they had little time for other concerns. From 1933 through 1935 Roosevelt enjoyed a good press in Germany, where parallels were drawn about strong leadership as opposed to parliamentary floundering, and Schröder sees in this some attempt to achieve a propagandistic legitimizing of the Nazi system. There was relatively little ideological conflict in these years, he finds, but gradually and almost unnoticed there developed a fundamental clash in economic policies. Just as the United States was undertaking the reciprocal trade-agreements program Germany turned away from the most-favored-nation principle to adopt Hjalmar Schacht's "New Plan" for barter and bilateralization. While Cordell Hull and Francis B. Sayre were saying over and over that world stability could come only through a fair expansion of international trade, Hitler and Goering were concentrating on autarky, armaments, and the Four Year Plan.

From 1936 on, German-American relations deteriorated rapidly. Schröder skimps the political side of the story to play up economic confrontation in Latin America and Southeast Europe. Even though one protagonist had surpluses and the other shortages, each needed foreign trade for its own inner stability, and in Schröder's view their collision of interests came more from economic forces than from ideology. He sometimes appears to think that a quotation proves a policy, but in general he marshals well the evidence for his debatable thesis. One small point: Norman Thomas would no doubt have been amused to find himself confused with Eugene P. Thomas as president of the National Foreign Trade Council.

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B. H. LIDDELL HART. *History of the Second World War*. New York: G. P. Putnam's Sons. 1971. Pp. xvi, 768. $12.50.

B. H. Liddell Hart had as much influence on the way in which World War II was fought as any single individual. He was the author of more than thirty books, which had enormous impact on military thinking about mechanized warfare, especially among the Germans, whose early success in the war came in large part from techniques taken from Liddell Hart's teachings. The German armored Blitzkriegs in 1939 and 1940, in turn, forced the Allies to adjust to the new methods of waging war. Although Hart's direct influence was always less in Britain and America than in Germany, several of Britain's wartime leaders turned to him throughout the war for advice and suggestions.

The *History of the Second World War* is, therefore, partly a commentary by the teacher on how well his pupils did in applying his concepts. Hart was fundamentally unhappy with the conduct of the war by the generals on both sides. His constant theme is lost opportunities due to excessive caution. Hart's comment on the Allied campaign in Italy is typical: "Favourable opportunities of faster progress were repeatedly missed through the Allied commanders' heavy emphasis on 'consolidating' each advance and establishing a 'firm base' before pressing on, together with their predominant concern to ensure ample strength and supplies before advancing. Time after time they were 'too late' from fear of having 'too little.'" There is scarcely an important soldier of World War II whom Hart does not fault for an unwillingness to accept risks.

Liddell Hart began working on the *History of the Second World War* in 1947; he was reading the proofs in 1970 when he died. It is possibly his most important book, ranking with *The Tanks* (1959), *Strategy* (first edition in 1929), *The Real War 1914–1918* (1930), and *The Other Side of the Hill* (1951). This is,

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indeed, the capstone to a life of intensive work. Hart kept extensive notes of discussions he had with British and foreign military officers; during World War II he interviewed the leading Allied participants about their decisions as soon after an event as possible; after the war he interrogated the German commanders while keeping up his contacts with British officers. In addition, he was thoroughly familiar with the scholarly literature on World War II. The result is a magnificently researched book.

This is exclusively military history. Indeed, the only major weakness of the book is the well-nigh total absence of politics. We gain no impression of why the nations fought, nor toward what end. For example, Hart devotes one paragraph to a meeting between Molotov and Ribbentrop in June 1943 at Kirovograd behind the German lines to discuss the possibilities of ending the war. Molotov wanted the prewar frontiers restored, while Ribbentrop held out for a frontier along the Dnieper River. The peace discussions ended when a report on them reached the Western Allies. Hart's only comment is, "The issue was then referred back to the judgement of battle."

Chapter 33, "The Crescendo of Bombing," is one of the best short accounts of the Allied strategic bombing campaign available (an earlier chapter on the Battle of Britain is also excellent). As in his earlier work Hart holds the RAF responsible for the indiscriminate bombing policy, points out that the British developed the program independently (Douhet's writings did not even appear in Britain until 1943), and maintains that the campaign was a military failure. He gives high marks to Lord Tedder for his concentration on transportation and communications targets in France before the Normandy landings and to Carl Spaatz for the oil campaign. He concludes that the air attacks had no significant effect on Germany's capacity to wage war until 1944, when the Americans introduced adequate long-range fighters to escort bombers. Even then, however, "Allied progress in the air, as on the ground, suffered from lack of concentration. The potential of the Allied air forces was greater than their achievement. In particular, the British pursued area-bombing long after they had any reason, or excuse, for such indiscriminate action." Concentration on oil and communication targets could have shortened the war by some months.

This is the first time Hart has dealt with the war in the Pacific in any detail. U.S. Navy submariners should be pleased with his conclusions; the American submarine campaign, he declares, was "the most important factor in Japan's final collapse—decisive in the way it exploited her economic weakness and dependence on overseas supplies." There was no need to use the atomic bomb and the Russian advance in Manchuria had as much effect on the timing of the Japanese surrender as did the bomb.

For British and American participants in the war, and for later historians, the greatest controversy centered about the campaign in northwest Europe. The key question was whether the advance should be via a strong, single thrust or on a broad front. Hart maintains that the war could have been won in September 1944 had Eisenhower opted for a single thrust—but only if he had given priority in supplies to George Patton's Third Army east of Paris. "Eisenhower's 'broad front' plan of advance . . . would have been a good way to strain and crack the resistance of a strong and still unbeaten enemy. But it was far less suited to the actual situation, where the enemy had already collapsed, and the issue depended on exploiting their collapse so deeply and rapidly that they would have no chance to rally. That called for a pursuit without pause." Montgomery's single thrust was better in principle, but it was frustrated by a number of factors—Montgomery's own caution, his delay in opening the port of Antwerp, the excessive provision of ammunition and other supplies that cut into the transport available for fuel, and other problems. The diversion of supplies to Patton was not a factor, as the Third Army got barely enough supplies to sustain itself. Hart maintains that "the best chance of a quick finish was probably lost when the 'gas' was turned off from Patton's tanks in the last week of August, when they were 100 miles nearer to the Rhine, and its bridges, than the British."

Hart touches on the more fundamental cause of the Allied failure to exploit the August victories in France in his concluding comment on the campaign: "The root of all the Allied troubles . . . was that none of the top planners

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had foreseen such a complete collapse of the enemy as occurred in August. They were not prepared, mentally or materially, to exploit it by a rapid long-range thrust." Just as, one might add, the Germans in 1940 were not prepared to take advantage of their stunning and surprising defeat of France by invading England.

This is an excellent, provocative history. It is hardly possible that there will be a one-volume military history of World War II to surpass it.

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MEDIEVAL

PETER HUNTER BLAIR. *The World of Bede*. New York: St. Martin's Press. 1970. Pp. x, 340. $10.00.

The author of this book is well known for his admirable text, *An Introduction to Anglo-Saxon England* (1956), and specialized writings have established his reputation as today's foremost student of the Old English North Country. Few scholars could have written an account of the life, times, and work of the Venerable Bede with the authority and sympathy that Hunter Blair demonstrates, and his book should prove popular and useful, especially to undergraduates.

A survey of the contents of so compendious a volume is impracticable; suffice it to say that mingled with general discussions of the historical, religious, and intellectual contexts of the age are numerous explorations of specific topics and problems from which anyone will learn something. Only the economic historian will find his interests neglected in this work, the title of which is otherwise so apt.

It is to be regretted, however, that a book whose merits recommend it as standard reading should be flawed by so many evidences of hasty or careless composition. Difficult as it is to understand how some sentences came to be written, it is incredible that they survived the scrutiny of readers and printer. On page 92, for example, we have the absurdity of the "infant Eanflaed . . . presiding, jointly with her daughter, over the monastery at Whitby. . . ." Equally nonsensical is the assertion on page 37 that Aethelbald of Mercia was "a representative of the only one of all the Anglo-Saxon royal families whose descent can . . . be traced from men who ruled near Schleswig before the migration of the English to Britain." What is meant, of course, is that the Mercian royal line is the only one that can be traced back to the Continent. Since it is the only one that originated "near Schleswig," the statement as it stands is silly.

Not so egregiously blundered but thoroughly blameworthy is the remark on page 123 that a certain Faustus "may possibly have been a native of Britain, though perhaps more probably of Brittany. . . ." To link "possibly" with "more probably" is deplorable; "perhaps" is intolerable! Repetition of information is also objectionable, although some of it presumably results from the author's attempt to give an integrity to each of the twenty-seven short chapters; but surely it is excessive to be told four times that Princess Eanflaed was the first Northumbrian to be baptized, and six times that Paulinus performed the rite. The number of misprints and petty slips is also inordinate, though most are innocuous. Aside from pointing out that on page 92 the two letters are from rather than to Boniface, I note, only because these are common errors, that Aryan (pp. 272, 275) should be Arian and Dionysius (p. 206) should be Dionysus.

Finally, the index is incomplete and erratic. Among significant omissions are Felix, author of the *Life of Guthlac* (pp. 109, 203, 280); *Gewisse* (p. 110); and vernacular (pp. 28, 234–35). Of two men named on page 103 and two places cited on page 109, one each is in the index. Under place names in the index there is no reference to page 32; yet of four examples found on page 32, three are in the index while the fourth is omitted. This is shoddy work.

R. D. WARE
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DAVID KNOWLES. *Thomas Becket*. (Leaders of Religion.) Stanford: Stanford University Press. 1971. Pp. xi, 183. $6.95.

Few men have maintained the integrity both of their scholarship and of their faith as has Professor Knowles. Lacking either, he probably could not have produced this sensitive, gracefully written biography, the result of more than twenty years of research and reflec-

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tion. A short book, *Thomas Becket* is concise, direct, and learned. The author's views of the participants have changed little over the years: Thomas remains admirable and forthright, Henry II is still "shifty, treacherous and obstinate," Gilbert Foliot embittered and ambitious, Roger of York scheming and grasping. I do not imply that Professor Knowles is partisan; he is too fine a scholar for that. As he explains, his earlier views have deepened and been confirmed by his rethinking of the Becket dispute. Nevertheless, he sometimes seems less than fair to Henry II and less critical of the archbishop than facts would appear to warrant and than he was in his short "character study" of Thomas published in 1949. These reservations are matters of opinion rather than of fact, of course, as is my view that Knowles is incorrect in dismissing the historic contest between Canterbury and York as essentially meaningless.

The book is well organized; some may feel that Thomas's career before becoming archbishop is slighted, but Radford's admirable study of that period in his life made it unnecessary for Dom David to traverse that ground very thoroughly. The only weak chapter is the second, which attempts too much in too limited a space: even Professor Knowles, with his great gifts, does not succeed in writing a good history of the English Church from the Conquest to Becket in eleven pages. The most interesting chapter is the last, a controlled, penetrating analysis of the dispute and its leading figures. The book concludes with a brief bibliographical essay.

Becket's great strength lies in its comprehensiveness: all the facts that we need to know about the archbishop are here, fairly and fully presented. Thomas's apparent change of character upon becoming primate is convincingly explained as well: "For the first time in his life he could put spiritual claims firmly before worldly interests [and he was] free to follow the call which he had long heard and neglected." Professor Knowles is rightly insistent upon the importance of the principles at stake in the Becket controversy, and he finds most of the Clarendon customs "ancient" but anachronistic. His narrative of the murder and its consequences is almost serene, neither melodramatic nor sentimental, and he finds that Thomas did not merit sainthood. The nature of the sources forbids a truly definitive biography of Thomas, but this wise little book is the best study of the man in any language. It takes a place of honor among other recent distinguished biographies of twelfth-century English churchmen: Hugh du Puiset, Hubert Walter, Anselm, Foliot, Theobald, Thurstan of York, Bartholomew of Exeter, and Aelred of Rievaulx.

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University of Georgia

MAISTRE NICOLE ORESME. *Le Livre de Politiques d'Aristote*. Published from the text of the Avranches Manuscript 223, with a critical introduction and notes by ALBERT DOUGLAS MENUT. (Transactions of the American Philosophical Society, New Series, Volume 60, Part 6.) Philadelphia: the Society. 1970. Pp. 392. $10.00.

Nicole Oresme died in 1382 as bishop of Lisieux and was buried in his cathedral next to the left entrance to the choir. Today, as Menut notes, there is no trace of his tomb. I likewise searched in vain for it in 1960 and can testify that at that time the clergy of his church seem never to have heard of him. Yet after Jean Buridan's death about 1358, Oresme, a peasant's son who became an intimate of Charles V of France, was the leading intellectual of Northern Europe, and perhaps of Europe.

Oresme's rehabilitation began with economists of the last century who recognized his *De mutatione monetarum* as the first treatise on the effects of debasing coinage. With Pierre Duhem historians of science discovered him, and by 1948 George Sarton could call him "one of the greatest mathematicians, mechanicians and economists of the Middle Ages." Meanwhile, students of the history of the French tongue had begun to find in Oresme a founder of French learned prose and a protean source of new vocabulary.

Now, thanks to Menut's edition of his translation, with commentary, of Aristotle's *Politics*, Oresme will become a figure in the history of political thought as well. To enlighten the secular officials of his court, Charles V commissioned Oresme to turn into French and to expatiate upon the *Ethics*, the pseudonymous *Economics*, the *On the Heavens*, and the *Politics* (tr. 1371). This was more than an educational enterprise: in 1372 and 1373, clearly as

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a reflection of Aristotle's advocacy of election of officials, Charles V permitted the royal council to elect the chancellor, and Parliament to elect its president.

A symbol of the interest of this text is the fact that in it such French words as *democracie, aristocracie,* and *olygarchie* appear for the first time. Oresme's glosses are often lengthy, and he shows an independent mind: twenty-five times he cites Albertus Magnus's commentary but almost always to differ with it. His ideas on church polity were quite conciliarist. In discussing population problems (*dépopulation* is one of his neologisms) he agonizes over methods of limitation, concluding that infanticide is worse than abortion, but that both are bad (p. 334). A long gloss on universal rulership (pp. 289–94), doubtless inspired by imperial claims and the English threat to the French monarchy, concludes that it is not feasible. He challenges (p. 298) the notion that politics is determined by climate: does not the decayed Greece of 1371 have the same climate it enjoyed in its greatest age? An elaborate gloss (pp. 302–04) examines the question whether a polity can exist without priests. Since in 1371 Oresme was dean of Rouen presiding over a chapter of seventy canons, his decision contra is not astonishing; but the detail and fairness with which he marshals the arguments pro show that the Joachimite expectation of the priestless Third Age was fermenting in the streets around his cathedral. Menut does not appraise Oresme's thinking, but from the author's own manuscript he has provided a text to which political theorists and historians will return often.

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J. LEE SHNEIDMAN. *The Rise of the Aragonese-Catalan Empire, 1200–1350.* In two volumes. New York: New York University Press. 1970. Pp. xv, 275; ix, 280–624. $20.00 the set.

By the end of the Middle Ages Spanish power had become the primary fact of political life in the western Mediterranean. During the fourteenth and early fifteenth centuries the Aragonese-Catalan state had firmly fixed its political and economic interests throughout the region, and the marriage of Ferdinand and Isabella had transformed these Aragonese claims into Spanish interests. The pursuit of these claims was to become a decisive factor in the dissipation of Spanish power and played a major role in shaping the development of the whole of early modern Europe. Considering the importance of the topic, relatively few British and American historians have devoted themselves to its investigation. Since the publication of H. J. Chaytor's *History of Aragon and Catalonia* in 1933, the only major works in English on the development of the Aragonese empire have been those of Kenneth Setton and Sir Steven Runciman, and, more recently, Ralph Giesey, Robert I. Burns, S. J., and Thomas F. Glick. Even these have been devoted to specialized topics. As a result there has been no single modern work in English with which to introduce students to the study of one of the most important states of the later medieval period.

Professor Shneidman's work could have been designed specifically to fill this need. Intended primarily as a study in political and constitutional development, these two volumes accomplish something more. A significant portion of the work provides a detailed résumé of the political history of Aragon during the period. Separate essays discuss the governance of the various realms of the Aragonese state, the theory and practice of central government, the activities of the Church, the position of the Jews, economic development, foreign policy, and fiscal organization. Each section generously provides the sort of background that is welcome to one not intimately acquainted with the field. The work concludes with a seventy-page bibliography divided topically: the Church, constitutional history, cultural and social history, economic history, foreign policy, the Jews, and political history. This compilation provides an excellent preliminary guide to the history of late medieval Aragon and Catalonia.

This breadth of treatment is not without its disadvantages, however. The author has felt compelled to adopt a rather complicated organization that not only often makes it difficult to follow the main lines of the discussion but also necessitates considerable repetition. He has apparently found it necessary to compress some of the background to the extent that complex and unresolved historical questions are sometimes presented in an oversimplified manner. These annoyances, however, do little to detract from the overall value of the work.

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On the other hand, the explicit theme of these volumes is "the desire of the kings to liberate themselves from the constitutional binds placed upon them by the military aristocracy." The emphasis is generally correct; the growth of the power of the central monarchy is the most important theme in the political history of late medieval Europe. It is also probably the most difficult to present in a balanced manner. Many historians have tended to view the growing power of the medieval monarchs as the wave of the future and the eventual triumph of autocracy as the culmination of an inevitable and desirable evolutionary process. The cards have been stacked against the local aristocracy who opposed the extension of royal power. They neglected to compile their claims in easily studied law codes; they represented the traditional agrarian interests of the age rather than those of a nascent capitalism; and most of all they failed in their struggle to limit the power of the central authority. This situation is especially evident in Spanish historiography, where political considerations have often made it difficult to confront with complete impartiality questions of local autonomy, constitutional limitations upon central authority, the power of popular assemblies, and the force of local law and custom.

The present work does little to improve upon the findings this limited approach has produced. It is a competent and forceful study, but completely within the traditional mode. The Crown is the paramount hero of the piece. The chapter entitled "The Economic Development of the Aragonese Empire" in reality describes the growth of the middle class as an ally of the monarchy. The lengthy discussion of the Jews is designed basically to illustrate "their motivation in serving the Crown." Every consideration seems subordinated to the growth of royal power, and little attention or sympathy is extended to the opposition to this power. This lack of balance is the most serious shortcoming of a substantial and long overdue addition to the bibliography of the field.

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PERCY ERNST SCHRAMM. *Kaiser, Könige und Päpste: Gesammelte Aufsätze zur Geschichte des Mittelalters.* Volume 3. (Beiträge zur allgemeinen Geschichte. Part 3, Vom 10. bis zum 13. Jahrhundert.) Stuttgart: Anton Hiersemann. 1969. Pp. 460. DM 98.

The present work is the third volume of a four-volume collection of essays and articles on problems in medieval history by the late P. E. Schramm (see his obituary, *AHR*, 76 (1971): 961–62). The title of the collection aptly reflects the author's lifework: five decades of indefatigable research through the highways and byways of the Middle Ages in general and of the Holy Roman Empire in particular, which made him among medievalists the undisputed master of medieval kingship and emperorship, are now suitably crowned by this florilegium. It is especially Schramm's brilliant studies of symbols and rituals and of liturgical and iconographic forms that raise this last collection of his works to the level of a further monumental contribution to our understanding of the full significance of the *Herrschaftszeichen und Staatssymbolik* to which he had devoted an earlier collection of his essays.

For a discussion of the first two volumes, see *AHR*, 75 (1969–70): 462–63. The third volume continues chronologically where the second stopped and covers the tenth to the thirteenth centuries. Most of the essays it contains have been previously published but have now been reworked and adjusted to the current state of the scholarship; one is new; all have up-to-date bibliographies. This publication of the essays supersedes therefore their previously published forms, and they should be used and cited as they appear here. Although all the essays are of the highest quality, they are necessarily disparate: they were originally written at different times for different purposes and for different audiences; their common denominator is necessarily as vague as their collective title. A collective assessment of the whole volume must therefore remain general, and the need for brevity precludes a discussion of each article individually. Nor is it fitting at this stage to sit in judgment over the work of the *doyen* of German historians: *a nemini judicetur*. To praise the work of one of the most eminent medievalists is superfluous and to animadvert each *lapsus calami* petty. The readers of this review are therefore better served by a list of the articles contained in the volume (the title is in each case a quite reliable indication of what the article covers) with references to

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their previous publication to facilitate indentification.

The volume contains the following works. Introductory: "Ein 'Weltspiegel' des 10. Jahrhunderts: Das 'Polypticum' des Bischofs Atto von Vercelli" (first published in *Zeitschrift für Rechtsgeschichte,* 49 Germanistische Abteilung [1929]: 180–98); Section A: "Ottos I. Königskrönung in Aachen (936): Die Vorakte und die Einzelvorgänge im Rahmen der deutschen Geschichte" (recast from *ibid.,* 55, Kanonistische Abteilung, 24 [1935]: 196–215, and *Geistige Arbeit,* Oct. 20, 1936, p. 5); "Der Ablauf der deutschen Königsweihe nach dem 'Mainzer Ordo' (um 960)" (from *Zeitschrift für Rechtsgeschichte,* 55, Kanonistische Abteilung, 24 [1935]: 233–74); "Die Königskrönungen der deutschen Herrscher von 961 bis um 1050" (recast from *ibid.,* 274–306); "Hofkapelle und Pfalzen" (six book reviews reprinted from various sources, mostly issues of *Geschichte in Wissenschaft und Unterricht*). Section B: "Die Kaiser aus dem Sächsischen Hause im Lichte der Staatssymbolik" (recast from *Mittheilungen des Instituts für Österreichische Geschichtsforschung,* supp. vol. 20, bk. 1 [1962]: 31–52); "Kaiser, Basileus und Papst in der Zeit der Ottonen" (recast from *Historische Zeitschrift,* 129 [1924]: 424–75); "Zwölf Briefe des byzantinischen Gesandten Leon von seiner Reise zu Otto III. aus den Jahren 997–998" (recast from *Byzantinische Zeitschrift,* 25 [1925]: 89–105); "Kaiser Otto III. (980–1002): seine Persönlichkeit und sein 'byzantinischer Hofstaat'" (first published in part in P. R. Rohden and Georgije Ostrogorsky, eds., *Menschen, die Geschichte machten,* 2: 8–14, and in part in *Kaiser, Rom und Renovatio* [Leipzig, 1929], 2: 17–33); "'Bonmots' mittelalterlicher Kaiser (Karl der Grosse, Otto III. und Heinrich IV.)" (not previously published). Section C: "Ein oft benutztes Lobwort: *spes imperii,* verwandt auf einer Bulle Kaiser Konrads II." (first published in *Festschrift für K. G. Hugelmann,* 2: 576–78); "Eine Echternacher Prunkhandschrift, wohl aus dem Besitz Kaiser Konrads II." (an augmented book review first published in *Historische Zeitschrift,* 185 [1958]: 376–80); "*Graphia aureae urbis Romae* (Kern von etwa 1030, mit den 'Mirabilia urbis Romae' verbunden um 1155)" (first published in *Kaiser, Rom und Renovatio,* 2: 68–111); "Die Schilderung der Krönung eines römischen Kaisers aus dem hebräischen Geschichtswerk des Josippon (wohl 1. Hälfte des XI. Jahrhunderts)" (recast from *ibid.,* 2: 112–19); "Heinrich III.: 1046 zum Kaiser gekrönt und investiert als *Patricius Romanorum*" (first published in *ibid.,* 1: 228–38); "Der 'Salische Kaiserordo' und Benzo von Alba. Ein neues Zeugnis des Graphia-Kreises" (recast from *Deutsches Archiv,* 1 [1937]: 389–407); "Der Abschnitt über *Roma, Romani* usw. aus dem Glossarium des Papias" (first published in *Kaiser, Rom und Renovatio,* 2: 137–40); "*Laus Caesaris Heinrici* des Azelin von Reims-Atto von Monte Cassino (zwischen 1047-1056)" (augmented from *ibid.,* 2: 141–47); "Zu Heinrich IV. (1056–1106), dem vorletzten Salier: Heinrich IV. und Rom" (reviews of collected essays from *Göttingische Gelehrten Anzeigen,* 207 [1953]: 87–92); "Eine wichtige Gestalt der späten Salierzeit: Die Gräfin Mathilde von Tuszien. Edition der *Notae de Mathilda Comitissa*" (first published in *Monumenta Germaniae Historica,* Script. 30, ii, 973–75); "Zu Heinrich V. (1106–25), dem letzten der Salischen Kaiser" (an augmented book review from *Geschichte in Wissenschaft und Unterricht,* 19 [1968]: 453 ff.); "Die Kaiseridee des Mittelalters (ein zusammenfassender Vortrag)" (recast from a lecture published in *Württembergisch Franken,* 41 [1957]: 3–16).

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MODERN EUROPE

ROLAND H. BAINTON. *Erasmus of Christendom.* New York: Charles Scribner's Sons. 1969. Pp. xii, 307. $6.95.

This portrait of Erasmus demonstrates once again Roland Bainton's capacity to write well for a general public while maintaining a high standard of scholarship. Scholarly issues are kept in the background, but Bainton's judgment is sound and informed. On points of detail there are many fresh observations of the kind only possible for one steeped in the sources. No one else has noticed, for example, that *Ecclesiastes* (1535) contains a précis of the antiwar speech Erasmus composed in Italy in 1508.

Bainton's general interpretation will not

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surprise those familiar with his sympathy for the free spirits of the Reformation era. He finds the essence of Erasmus in a spiritual religion that he learned from the Brethren of the Common Life and that was confirmed by Neoplatonism and by his own study of the New Testament. He never approved burning for heresy nor defended authority for its own sake. Rather, as is clear from his attack on the late scholastic conception of *potentia absoluta*, he believed that absolute power corrupts even God.

Erasmus was indeed a free spirit, but Bainton is too eager to make him the exponent of a purely spiritual religion. Quoting a statement by Erasmus that he would rather die than desert his Catholic friends, Bainton says of Erasmus's Catholicism that "personal attachments may lead to rationally indefensible behavior" (p. 196). But the statement actually says that Erasmus will not become "factious" (that is, schismatic) even to please his many learned friends, whom he would otherwise rather die than desert. He often departed from the *consensus* of the Church, as Bainton says, when discussing the moral issues that mattered more to him than dogmatic questions. But one cannot therefore disregard his repeated declarations that, despite a certain sympathy for the spiritual interpretation of the sacrament put forward by Reformers in Basel, he felt bound in conscience to abide by the *consensus* of the Church on the Eucharist.

The present scholarly generation tends to exalt Erasmus as a man of peace, just as the previous generation insisted on condemning him for his supposed moral cowardice. According to Bainton Erasmus never faltered in his liberal convictions. This view obscures in Erasmus the ambivalence toward authority that was characteristic of Renaissance humanists. Might it be permissible as well as wise, for the sake of public order, to deceive the multitude about the authority of popes and prelates? Ignoring passages that discuss the Platonic lie, Bainton dismisses too quickly the suggestion that the late Erasmus had reactionary tendencies. Erasmus of Christendom has partly overshadowed Erasmus of Rotterdam.

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HELMUT BACKHAUS. *Reichsterritorium und schwedische Provinz: Vorpommern unter Karls XI. Vormündern (1660–1672).* (Veröffentlichungen des Max-Planck-Instituts für Geschichte, Number 25.) Göttingen: Vandenhoeck & Ruprecht. 1969. Pp. 308. DM 32.

The title of this book seems to promise a history of Swedish Pomerania, 1660–72, both as part of the Holy Roman Empire and as a Swedish province. Only the second part of the promise is partially fulfilled. We learn next to nothing about the *Reichsterritorium*. Backhaus focuses upon the government of Swedish Pomerania and its juridical, administrative, financial, and political relationship to the Swedish Crown. He describes the government, its subdivisions, nomenclature, processes, finances, and personnel, its presumed legal bases, and its practical power bases.

Backhaus repeatedly points out, quite correctly, that the regency council then governing Sweden treated Pomerania with considerable delicacy and restraint—notably in fiscal matters. Neither his narrative nor his analysis, however, clearly accounts for it. The fact that Pomerania was *Reichsterritorium* is noted at frequent intervals, but the limitations that status imposed—for example in fiscal matters—are not spelled out, and other reasons could well be as important. The particular interests and activity of Governor-General Wrangel, the factional in-fighting in Stockholm, and the outright bribery, are all elements in the story Backhaus provides us, without evaluation. More important, perhaps, are the elements he does not provide. Pomerania's budgets are discussed without reference to population, social problems, resources, or the economy as a whole. There is no real effort to compare Pomerania's fate to that of other lands under the Swedish Crown or to other territories in the Empire, Bremen-Verden excepted. There is no basis for judging whether Swedish exploitation was severe or moderate or how it was skewed.

An expanded dissertation, the book has an impressive scholarly train, and the footnotes are as interesting as the text. In spite of some curious omissions Backhaus seems well in command of the literature. He also presents fairly extensive tables pertaining mainly to the financial affairs of the provincial government. Unfor-

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tunately he does not succeed in making the tables clearly understandable or very relevant to either text or thesis.

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KARL-HEINZ OSTERLOH. *Joseph von Sonnenfels und die österreichische Reformbewegung im Zeitalter des aufgeklärten Absolutismus: Eine Studie zum Zusammenhang von Kameralwissenschaft und Verwaltungspraxis.* (Historische Studien, Number 409.) Lübeck: Matthiesen Verlag. 1970. Pp. 271. DM 36.

PIERANGELO SCHIERA. *Il cameralismo e l'assolutismo tedesco: Dall'arte di governo alle scienze dello Stato.* (Archivio della Fondazione Italiana per la Storia Amministrativa. First Series, Monografie, ricerche ausiliarie, opere strumentali. Number 8.) Milan: Dott. Antonino Giuffrè. 1968. Pp. 473.

The trouble with cameralism, at least for Americans, is that so much underbrush has to be cleared away before one can begin discussing it usefully. Unfortunately, neither Schiera's nor Osterloh's work, for all the admirable qualities each demonstrates, is likely to clarify the misconceptions concerning cameralism that are so plentiful this side of the Rhine.

Joseph von Sonnenfels, the subject of Osterloh's book, was a bold, polemical writer, a hard-working administrator, and a highly successful teacher who placed his students in university *Kameralwissenschaft* chairs throughout the Habsburg lands and who knew how to hold his own when challenged by reactionaries seeking to topple him from favor. He was one of the chief cameralists of his day.

After a short introduction on the forces working for reform in Austria after 1740, Osterloh gives us a section dealing with Sonnenfels' main theories on local administration, commerce, and fiscal affairs. While there is little new or surprising here, Osterloh's account is more detailed and better presented than any hitherto available. The second section takes up Sonnenfels' important contributions to Austrian reforms: unlike the first section, here Osterloh's findings are based to a great extent on archival materials and on his own reading of eighteenth-century publications rather than on the work of other scholars.

Schiera's book is made up of three grandly conceived and exhaustively executed essays: the historiography of cameralism from the late eighteenth century to the 1960s; the manner in which cameralists fused general political philosophy and precepts concerning administration into a new sort of political science; and the characteristics of late or "university cameralism." The main portions of this work are preceded by Schiera's rather restricted definition of cameralism, one that leads him to dismiss as "mercantilism" much of what most of us will stubbornly insist on calling cameralism. He also gives us a highly useful, almost forbiddingly long bibliography on the literature of cameralism—thirty-six quarto pages in small type.

Schiera's argument is that cameralism arises from the need to present "absolutist" princes of the German-speaking lands with both the rationale for their increasing powers and the techniques for training the new sorts of officials necessary for governing in the new fashion. This is an important concept and deserves our attention. But the style and organization of his book, I am afraid, will not endear it to historians. It seems addressed, rather, to political scientists with an interest in the history of ideas—though Schiera insists, somewhat unconvincingly, that he also is demonstrating the historical change in the ideas that accompanied the rise of the modern, centralized, all-powerful state. His work is too abstract, too discursive, too wearyingly concerned with the often inelegant and overlapping thoughts of half-forgotten commentators on government.

Neither of these books presents its findings from the point of view of today's concern with modernization. But both provide indirect support for the idea that the main reason cameralism was able to win a new lease on life in eighteenth-century Central Europe is that it was able to absorb certain Enlightenment concepts and attitudes, making it a highly useful instrument for promoting only those changes that strengthen the power of the ruler without seriously undermining the position of the aristocracy. For all its emphasis on reform, cameralism may have done as much to arrest modernization as to promote it.

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NORMAN ITZKOWITZ and MAX MOTE, annotated and translated by. *Mubadele—An Ottoman-Russian Exchange of Ambassadors.* Chicago: University of Chicago Press. 1970. Pp. x, 261. $11.50.

This edition of translations of the Ottoman and Russian reports of the formal embassies that were exchanged following the conclusion of the Treaty of Kuchuk Kainarji in 1774 illustrates some advantages and disadvantages of interdisciplinary, collaborative scholarship. Though the authors, a linguist and a political scientist, admit that they "have not exhausted the research possibilities that this confrontation between the Ottoman and Russian empires affords" (p. ix), their lack of familiarity with the historical literature, especially the work of Soviet scholars such as O. P. Markova, A. M. Stanislavskaia, G. L. Arsh, V. G. Sirotkin, and A. L. Shapiro, produces an unbalanced setting in the fifty-page introduction. The lengthy glossary that is appended is helpful for some names and Turkish terms, but omits obvious items while devoting inordinate space to others (for example, ranks and quarantine).

The two documents are presented separately with little effort at integration. In fact, they are quite different. The *sefaretname* (report) of Abdülkerim Pasha, actually written by a secretary attached to the Turkish mission and submitted to the sultan as prescribed by tradition, provides an interesting and valuable insight into Ottoman diplomatic formalities and describes the journey and reception by the court in Moscow. An Ottoman view of Russia in 1775 is a welcome contribution to historical literature, though it must be used with caution since the Turks apparently thought that they had "gone through the looking glass" at the frontier.

Prince Nikolai Repnin led the parallel mission to Constantinople and in 1776 and 1777 published his account of it, which "has been ignored by everyone but meticulous bibliographers." Perhaps for good reason, as the reader wanders tediously from one passing of coffee and pipes to another and searches in vain for the promise that it contains "an important amount of information pertaining to Russian expansion toward the Black Sea" (p. 31). Repnin appears to have been more interested in assuring that his long lists of courtiers were complete.

Since the embassies were primarily ceremonial, the diplomatic historian will find little in their descriptions that will illuminate the larger issues of Ottoman-Russian relations. What one does gain, however, is an atmosphere of court regalia and procedure that is so often forgotten or ignored in the history of the period.

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FRANK G. WEBER. *Eagles on the Crescent: Germany, Austria, and the Diplomacy of the Turkish Alliance, 1914–1918.* Ithaca: Cornell University Press. 1970. Pp. x, 284. $9.75.

Much of the ground covered in this study has been explored and described in Ulrich Trumpener's *Germany and the Ottoman Empire, 1914–1918* (1968) (reviewed in *AHR*, 75 [1969–70]: 821–22). Weber's study is based on a meticulous examination of the Austrian and German archives and additional materials from the Public Record Office and the Department of State. (Incidentally both Trumpener and Weber were denied access to the Turkish foreign ministry archives.)

With regard to the German position and the value of the Turkish alliance to the Central Powers, Weber's conclusions are substantially in agreement with Trumpener's —that the Germans failed to convert the hastily concluded alliance into a "horse-and-rider" relationship and that German control of Turkish policy and strategy was greatly exaggerated by contemporaries and later by scholars. Within the bounds of the alliance the chauvinistic Young Turks were the exploiters rather than the exploited. Although Weber covers fully the German side of the triangle, he has made his principal contribution in exploring the Austrian position in the alliance. His evidence shows that an alliance with Turkey was an Austrian project before the plunge into war in 1914. When Turkey became a belligerent but rejected the Austrian suggestion that she mount an offensive north from the Black Sea, Austria's interest in the alliance cooled and became henceforth mainly diplomatic. She had no direct interest in promoting

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the Egyptian or Mesopotamian campaigns or the Transcaucasian operations. The Turkish defense of the Straits and the victory at Kut-el-Amara in Mesopotamia greatly increased the Turks' self-confidence and independence toward their allies in matters of war policy and strategy. Moreover, the Turks had their own war aims, and they were not those of Austria or Germany.

The Marquis von Pallavicini, Austrian ambassador and dean of the diplomatic corps in Constantinople, was an astute tactician with a good understanding of the forces at play in Southeastern Europe and an unparalleled knowledge of the workings of the Ottoman government. By 1917 the persistent deception and measures of petty rivalry indulged in by the Austrian representative and his government had brought Berlin and Vienna into disagreement on every question concerning the Turkish alliance. The Ottoman leaders exploited this situation to the limit. All this is set forth by Weber in detail. There is also much about abortive peace feelers and grand designs for remaking the map of the Balkans and the disposition of other people's property —a wholly fruitless exercise as it turned out, but one indulged in by all the governments enmeshed in World War I alliances. The author judges Austrian policy to have been in general more realistic and steadier than the German, although in the course of the war "the alliance of Turkey and the Central Powers became a self-defeating proposition" (p. 262). The study has some tedious stretches, but its organization, style, and technical aspects are exemplary.

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W. K. JORDAN. *Edward VI: The Threshold of Power. The Dominance of the Duke of Northumberland.* Cambridge, Mass.: Belknap Press of Harvard University Press. 1970. Pp. 565. $11.50.

This second volume of Jordan's history of the reign of Edward VI, the least known and least understood of the Tudor monarchs, successfully completes the study so well begun in *Edward VI: The Young King.* Although this volume carries the subtitle *The Dominance of the Duke of Northumberland,* it is no simple chronicle of how a ruthless man, manipulating the whims and unformed personality of a boy-king, bent England to his selfish designs. Jordan considerably revises Froude's and Pollard's accounts of Edward's reign. He shows that Northumberland conducted much of his rule under the shadow of his rival, the duke of Somerset, even after the latter's execution in January 1552. Most significantly he portrays Edward, despite his boyish interest in pageantry and his poor health, as a Tudor monarch who at least "in his last year bore far more direct responsibility for royal decisions than had his father during his first royal year." Indeed Jordan explains the plan to pass over the order of succession established by Henry VIII's will not as a plot devised by Northumberland but as "the stubborn determination" of Edward himself who by personal imperious action coerced the Council into accepting it and thus "engulfed Northumberland in a gigantic treason." (See Jordan, pp. 514–17, and cf. Pollard, *The History of England from the Accession of Edward VI to the Death of Elizabeth* [1923], pp. 80–86).

The credibility of this revision of English history depends not on new evidence unavailable to Pollard but on a reinterpretation of the evidence based on a careful and detailed re-evaluation of the characters and policies of both Edward and Northumberland. The narrative of Jordan's whole second volume contributes to this comparative study. The chief new element is a more serious examination than anyone heretofore has undertaken of the writings and actions of Edward VI himself, especially his *Chronicle* and his discourses on policy. It hangs almost as much on a new view of Northumberland that is derived from a close survey of his acts and motives and that concedes to him for all his ruthlessness and deviousness at least a peppercorn of human loyalty to the king and to his class, if not to the realm as a whole. He appears, therefore, in this balanced judgment as a man caught up in events, forces, and fears only partly the result of his own overweening ambition and conspiratorial nature and not as an evil spirit incarnate.

Jordan's contributions are not limited to this reassessment of Edward and Northumberland. The three chapters on the development of Protestant polity—nearly one-third of the book—are notable, first, for clarifying the rela-

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tion of English religious developments with those on the continent, second, for revealing the roots of later Puritan dissent, and, third, for summarizing the effects of religious and political policies on the wealth and power of the Church and its chief officers, the bishops. Of special interest is his excellent study of the role of the pulpit in these years as exemplified in the preaching of men like Latimer, Becon, Hooper, and Lever. Closely related to these chapters are the two excursuses of the volume—one on "the dismantling of the Roman Catholic faith" and the other on secularism as seen in changing charitable impulses. This second excursus, however, leaves one wishing for a more comprehensive discussion of the growth of secularism than Jordan provides. When one thinks of the secularization of careers experienced by Sir Thomas Smith and his like, he realizes a need for a study of secularism in Edward's reign that goes beyond Jordan's long-standing interest in philanthropy. Finally Jordan's chapters on the weakening of England in foreign and military affairs and on the general crisis in the economy provide greater detail and depth of understanding than is easily available elsewhere.

Jordan's account of Edward VI's reign ends with an assessment of the character of the king but without a judgment on the place of the reign as a whole in English history. The loss of power and influence in foreign affairs, the weakening of military strength, the development of a Protestant polity and faith, the rise of secularism, and a state of general social and economic crisis are all themes that need to be put in the perspective of a longer view than Jordan gives in this book. In this sense his work seems incomplete. Perhaps that judgment cannot be made, or can be only tentatively made, without a contrapuntal study of the reign of Mary Tudor. For one, I would encourage Professor Jordan to undertake such a companion work and then give us an overall evaluation of the significance of the two reigns to English history.

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HOWARD S. REINMUTH, JR., editor. *Early Stuart Studies: Essays in Honor of David Harris Willson*. Minneapolis: University of Minnesota Press. 1970. Pp. viii, 271. $9.50.

These essays honor Professor Willson by attesting to the value of his scholarship both as a major contribution to the history of early seventeenth-century England and as a stimulus to further research. Most of the essays reflect Willson's preference for political history and biography, and hence the book does not offer a wide variety of approaches to the period. Yet the authors do respect the social context within which political developments occurred, and Robert C. Johnson deals directly with social problems in his brief study of the transportation of vagrant children to the New World by the Virginia Company and the City of London. John K. Gruenfelder makes convincing use of statistics in his analysis of the elections to the Short Parliament.

Many of the essays are individual case studies, and consequently the volume appears to lack a unifying theme. Yet one purpose of the book is to emphasize the distinct character of the early Stuart period, a task that is more agreeable to political than to economic historians. Five of the studies deal with the development of political tension after the accession of James I, whom Willson has so vividly portrayed. W. J. Jones approaches the problem through the perspective of Lord Chancellor Ellesmere, whose orthodox and legally inspired political principles sufficed during the reign of Elizabeth but left him unequipped to settle early seventeenth-century disagreements between the king and the House of Commons. Robert Cecil, on the other hand, emerges from Thomas M. Coakley's essay as a man who adapted more easily to the politics of the Jacobean age but who failed nonetheless to find a solution to the central problem of royal finance.

The essays by Jones and Coakley illustrate the importance of studying James's administrators in order to explain the political problems of his government, a point that John H. Barcroft makes explicit in his examination of Sir Dudley Carleton's attempt to gain office. He qualifies the traditional criticism of crass venality in the sale of offices, but still concludes that the complexity of patronage during the ascendancy of the duke of Buckingham impeded the effectiveness of personnel decisions. Buckingham also comes under attack in Arthur P. Kautz's study of the selection of Jacobean bishops. James himself was respon-

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sible for appointing religious controversialists to the episcopal bench and allowing them to become prominent in affairs of state. But the period of Buckingham's influence and the laxity of episcopal government under George Abbot draw the author's sharpest criticism.

The essay by Gruenfelder discusses the development of political tension at the end of the early Stuart period. Through a systematic analysis of electoral patronage he demonstrates how the government suffered a defeat in the spring election of 1640 as the result of both the unpopularity of its policies and the superior organization of the reform group.

Two essays deal in different ways with the position of Catholics in England. Joel Hurstfield investigates the Gunpowder Plot and shows how it affected the fortunes of even those Catholics who were loyal to the government. He concludes his study with a broad interpretation of religious dissent, both in England and in Europe. Howard Reinmuth examines the efforts of the first two Stuarts to fill the power vacuum created by the disappearance of the great northern families at the end of the sixteenth century. He illustrates through the example of Lord William Howard how a Catholic who was not proceeded against for recusancy could exercise considerable political power at the local level.

This volume makes a noteworthy contribution to the historical literature of the period. The quality of the individual essays is somewhat uneven, but they are all original, thoughtful, and well written.

BRIAN P. LEVACK
University of Texas, Austin

ROBERT ASHTON *et al. The English Civil War and After, 1642–1658.* Edited by R. H. PARRY. Berkeley and Los Angeles: University of California Press. 1970. Pp. viii, 127. Cloth $7.00, paper $2.45.

This small volume contains seven essays, six of which were delivered as lectures at Eton College: "The Outbreak of the English Civil War" by Brian Manning, "The Rebels of 1642" by D. H. Pennington, "The Trial of Charles I" by C. V. Wedgwood, "Oliver Cromwell and the Rule of the Saints" by Austin Woolrych, "Swordsmen and Decimators—Cromwell's Major-Generals" by Ivan Roots, and "The Civil War and the Class Struggle" by Robert Ashton. To this list Ivan Roots has added a concluding essay on "Interest—Public, Private and Communal."

One of the better essays is this second one by Mr. Roots, who brings together threads from several of the lectures. A striking feature of this and some other essays is the emphasis placed on behavioralistic or motivational considerations, although this is more implicit than explicit. A second prominent feature of this and some other essays is the stress placed on community, real or imagined, and the complex overlapping of communities. In varying degrees Ashton, Pennington, and Roots make use of the expanding and revealing studies of communal relationships. These relationships often took on the legal and philosophical cloak of the common law on property before they acquired political and constitutional status. I, for one, am delighted to hear Mr. Ashton suggesting an interrelationship between an individual's religious position and his economic community and success or failure therein. It is also pleasing to hear Mr. Roots saying that even the gathered churches began seeking and acquiring a communal character that went much beyond theology. Little of this is entirely new, but rarely has it been presented so succinctly or so effectually, nor with so much promise of things to come.

In a very different category is Miss Wedgwood's lecture on the trial of Charles I. It is an excellent condensation of her widely acclaimed book on the same subject, and it should be read by every undergraduate student.

Somewhat less removed from the main thrust of the volume are the lectures by Woolrych and Pennington, and the first by Roots. Two of these hold forth promise of longer works. Woolrych's lecture on the Nominated or Barebone's Parliament raises any number of intriguing questions and provides a number of answers, some of which remain tentative pending completion of his larger study. Pennington's essay calls attention to the rather comic features of the king's appeal to the medieval past: raised standards and feudal arrays were not so appealing as the newer associations and loyalties. Like Roots, Pennington stresses the complex and conflicting motives and emotions

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that led men to act and react as they did, but more than this he deals with the reluctant groping of the rebels for a satisfying rationale for the situation in which they and the king found themselves. This is a topic in need of further examination. The major contribution of the essay by Roots on Cromwell's Major-Generals is its conclusion that all too little is known about the mid-1650s.

One of the more exciting of the lectures is that by Brian Manning, whose promised study on Civil War parties should be very rewarding. His major thesis is that whatever their feelings about individual issues the members of Parliament were divided primarily by the matter of trust or distrust of the king rather than by some motivating philosophy and that there were varying degrees of trust and distrust within the several factions, no one of which was eager to go to war but went to war nonetheless.

All of the essayists agree that there is no simplistic answer as to why individuals and groups acted as they did at any point, though there is an inclination to stress the presence and importance of individualism, localism, and special communalism. One can only conclude that Civil War historiography is in for another round of revisionism and that the revisionists are very able.

GEORGE R. ABERNATHY, JR.
University of North Carolina, Charlotte

ROBERT LATHAM and WILLIAM MATTHEWS, editors. *The Diary of Samuel Pepys: A New and Complete Transcription*. Volume 1, *1660;* Volume 2, *1661;* Volume 3, *1662*. Berkeley and Los Angeles: University of California Press. 1970. Pp. clii, 348; xi, 266; xi, 328. $27.00 the set.

These three volumes are the first installment of an eleven-volume edition; the remaining volumes are to be published during the next few years. Although nearly a century and a half has passed since the first printing of the diary, there has never been a complete edition. The first, Braybrooke's (1825), includes only about a quarter of Pepys's text, while even Wheatley's (published in the 1890s and heretofore the standard) sometimes omits substantial passages, and not only those of an erotic or scatological nature. Apart from being incomplete the earlier editions are not reliable, and, what with their misreadings, interpolations, and bowdlerization, the present editors show them little mercy. Braybrooke's edition they describe as "a travesty of the original," Mynors Bright's (1875–79) as not much better, and Wheatley's as revealing "serious lapses . . . in abundance on every page." To amend this situation they and several contributing editors have undertaken a complete edition of the text, which runs to around 1,300,000 words, about the same length as Gibbon's *History*. This new edition will fill nine volumes, each except the last covering a single year. The tenth volume, to be known as the "Companion," will be devoted to a collection of reference material, mainly articles on subjects that particularly concerned Pepys, but also including maps, genealogical tables, and a large glossary. The eleventh volume will contain the index.

About a third of the first volume is devoted to introductory comment. This includes a biographical essay on Pepys and essays on the diary as literature and as history. Further sections provide a detailed description of the manuscript and discuss the editorial policies employed in producing the present text. Since the manuscript is largely in shorthand, exact reproduction is out of the question. Punctuation is of necessity almost entirely editorial, though the paragraphing is that of the manuscript. The editors have seen fit to resort to modern British spelling, except for cases in which the shorthand indicates a variant pronunciation suggested elsewhere in Pepys's longhand. The longhand words remain as Pepys wrote them, save for the expansion of certain abbreviations and the elimination of a few archaic practices, such as writing the letters *ff* for *F*. The spelling is therefore mixed. As the editors point out, the text is a reconstruction rather than a reproduction. But it is, in their opinion, the only form "which will meet the legitimate demands of literary and linguistic scholars as well as historians, and at the same time be as nearly as possible true to the diary as Pepys wrote it." Purists may complain that the longhand has not been differentiated from the shorthand, save by italics in certain cases (headings, titles of books, names of ships, and the like), but the editors deemed it more important to avoid an "ugly, uneasy type-page."

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For this the great majority of readers should be grateful.

When the first edition appeared Sydney Smith described the diary as "nonsense," and Creevey thought it "almost trash." But Sir Walter Scott praised its richness of detail on the manners of the age, while the *Edinburgh Review* spoke of the diarist as having the most "indiscriminating, insatiable and miscellaneous curiosity that ever prompted the researches, or supplied the pen, of a daily chronicler." Few, if any, in later generations would gainsay its extraordinary value as a combination of private journal and public chronicle: certainly not the present editors. According to Professor Matthews, "no one else has ever composed so brilliant and so full an account of an actual man as he actually was," and Mr. Latham observes that, should all other records of the era be lost, "it would still be possible to reconstruct much of Pepys's world from the diary alone."

The portrayal of Pepys to be found in the introductory comment is on the whole a familiar one. The Pepys of the diary was a young man who as government official and private citizen enjoyed a wide variety of contacts in London. He was a civil servant who was building a reputation for expertness. He was a man of immense zest and energy; his gusto Matthews finds equaled, among English writers, only by Chaucer. And he was a *virtuoso* whose curiosity never slept. The present unexpurgated edition impresses us all the more with his frankness, a quality that suffuses the diary—and made it impossible for Victorian editors to print it in full. In dealing with Pepys's motives and methods in keeping his diary the editors are able to say more that is new, or at least not widely recognized even by seasoned students of Restoration literature. Aware that motivation is an elusive factor, and sometimes many-sided, they point to Pepys's orderly instincts, to Pepys the "accountant," taking satisfaction in setting down the business transactions, household economy, and social engagements of the day. They see him as something of a puritan (his "gusto" notwithstanding), using the diary for self-examination in the hope of self-improvement, and as the *virtuoso*, recording for his own edification the current manifestations of scholarship and science. They see him, too, as a would-be historian determined, in particular, to chronicle the political transformation of the Restoration; they even find in the diary traces of a feeling for literary creativity that we would associate with the works of a novelist.

The diary, it is clearly demonstrated, is much less artless than has been commonly supposed. The editors have found unmistakable evidence that some parts were originally written as rough notes and a strong possibility that others once existed in a presumably rougher form, as well as "abundant witness" that Pepys sometimes read over his entries and revised them. They suggest that the composition of the diary may have gone through as many as five stages and note that Pepys corrected his shorthand on about four thousand occasions. We can but speculate on this attention to literary refinement. That Pepys could have foreseen, even dimly, the publication of his diary is incredible. That he valued it not only for himself but as something of possible use for posterity is strongly indicated by its inclusion in the library of books and manuscripts that he presented to Magdalene College, specifically for the benefit of scholars. They, and others not so scholarly, can now begin to avail themselves of an edition that will remain the standard for many years to come.

WILLIAM L. SACHSE
University of Wisconsin, Madison

DAVID GREENWOOD. *William King: Tory and Jacobite.* New York: Oxford University Press. 1969. Pp. x, 386. $10.50.

One doubts that the name of William King (1685–1763), principal of St. Mary Hall, Oxford, will have much meaning for most historians. A few will be familiar with his posthumously published *Political and Literary Anecdotes of His Own Times* (1818), which contains some useful anecdotes about Swift and Pope and their circle and some amusing if apochryphal stories about Sir Robert Walpole. The obscurity into which his career has fallen cannot be much retrieved by Mr. Greenwood, for relatively few facts about King's life are available—his role as a college principal, for example, cannot be reconstructed. The author has made excellent use of King's autobiographical

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Anecdotes and of a small surviving correspondence, largely with John Boyle, fifth earl of Cork and Orrery, a correspondence that is now divided between the Bodleian and Houghton Libraries. King was a colorful anachronism, an ardent Jacobite, engaged for some years in clandestine political activity, and writing in sentimental support of the exiled Stuarts until the death of George II. His reputation rested otherwise on his elegant orations and poems in Latin, even then a dying art. Most of his works were written in reference to contemporary events, with a heavy pseudo-critical apparatus and with deliberately obscure allusions for which only a few partial and questionable keys have survived. A number of his tracts are devoted to petty quarrels in which he was engaged at the university and to a long lawsuit in Ireland in which he figured. Although Mr. Greenwood quotes extensively from King's writings, he compromises his efforts to obtain a wider appreciation for them by failing to provide translations. For an account of King as a university politician and of the society in which he functioned, W. R. Ward's *Georgian Oxford* (1958) is still the best source, especially as Greenwood does not seem perfectly informed about contemporary English parties and politics. The chief value of the book lies rather in a learned and painstaking examination of the writings of "the last example of a Latin poet in the grand manner" (p. 352).

HENRY L. SNYDER
University of Kansas

DAVID GREEN. *Queen Anne*. New York: Charles Scribner's Sons. 1970. Pp. 399. $8.95.

"Queen Anne is dead" has become a proverbial phrase signifying the passing of an age. But Mr. Green's concern is not with the realm and people over whom Anne ruled but with the personality and problems of the long-suffering queen herself. This volume is presented, then, as a popular biography by an author who has previously produced similiar works upon Sarah, duchess of Marlborough, Grinling Gibbons, and Henry Wise. His skillfully drawn portrait of the queen is one of a very ordinary but conscientious and stubborn gentlewoman almost martyred by poor health, personal tragedy, and the burdens of monarchy. She was not, in Mr. Green's eyes, the great queen that Winston Churchill would have had her, but her views could never be ignored by her ministers nor were her loyalties and prejudices easily overcome.

Mr. Green's picture of the queen is solidly founded on his examination of the main published and manuscript sources—at Blenheim and elsewhere—for the personal history of the reign, and he has drawn upon the recently unearthed diary of the royal physician David Hamilton to illumine Anne's state of mind in her closing years. He has also sought to shape his account in conformity with recent scholarship on the political history of the reign, and, as he tells us, his proofs have been subjected to the careful scrutiny of Geoffrey Holmes. His treatment of Anne and her court under William, however, might have benefited from similar professional assistance. In sum, *Queen Anne* can be recommended, at least to those who are fond of the genre, as a well-written, amply documented, and generally convincing study of the life and character of the last of the Stuarts.

HENRY HORWITZ
University of Iowa

Chain of Friendship: Selected Letters of Dr. John Fothergill of London, 1735–1780. With introduction and notes by BETSY C. CORNER and CHRISTOPHER C. BOOTH. Cambridge, Mass.: Belknap Press of Harvard University Press. 1971. Pp. xxiv, 538. $20.00.

Some two hundred letters, about half the known total, written by the Quaker physician and philanthropist, Dr. John Fothergill, are printed here in chronological order. Many appear for the first time, others from surviving copies or published texts, corrected where necessary. A brief biographical introduction, a map of contemporary London, illustrations, useful notes at the end of each letter, an appended genealogy, a bibliography, and an index including subjects and persons mentioned make this volume a welcome addition to the available documents of eighteenth-century life and thought. A separate list of correspondents might have been worth adding to the critical apparatus.

The majority of the letters were written to members of the Fothergill family. Some were

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addressed to professional colleagues and scientists—among these Linnaeus, who had named a specimen of witch hazel after the doctor. Recipients connected with Pennsylvania included the Bartrams, Benjamin Franklin, James and William Logan, Humphrey Marshall, John Morgan, Israel and James Pemberton, Richard Peters, Benjamin Rush, William Shippen, and Benjamin Waterhouse. Fothergill also wrote to Lionel Chalmers of South Carolina and Cadwallader Colden of New York.

As troubles developed between the colonies and England, Fothergill became actively involved in attempted reconciliation. He wrote to his fellow Yorkshireman, the marquis of Rockingham, and to his patient, the earl of Dartmouth, proposing to the latter as early as 1765 a conference to resolve Anglo-American difficulties (p. 249). He testified to Franklin's splendid efforts (p. 256) and to those of the "quadrumvirate," Barlow Trecothick, Capel Hanbury, David Barclay, and Daniel Mildred, as "incessantly laborious" in the same cause (p. 263). The last letter by him here, written shortly before his death and months before peace came, concerned among other matters the possibility of measures to preclude the necessity of general wars (p. 498).

Many topics besides politics fill the pages. Illness and its problems naturally interested the doctor and his friends. He had much to do with the Pennsylvania Hospital and with the establishment of other medical institutions. He collected coins and proposed the setting up of a general standard of weights and measures (p. 498). A good Quaker, Fothergill served the Society of Friends in many ways—as their correspondent with the Philadelphia yearly meeting; as a visitor to northern English meetings; twice as clerk to the London yearly meetings; as founder of the Ackworth school in Yorkshire. He also wrote to Franklin against the imposition of oaths and supported the scientific work of Joseph Priestley. Botany was a lifelong study; Fothergill hoped to alleviate the possible languors of old age with the care of his collection of native and exotic plants.

This is a delightful and valuable book, recommended for students and for browsers.

CAROLINE ROBBINS
Rosemont, Pennsylvania

HOWARD T. FRY. *Alexander Dalrymple (1737–1808) and the Expansion of British Trade.* With a foreword by R. A. SKELTON. (Imperial Studies Number 29.) [Toronto:] University of Toronto Press for the Royal Commonwealth Society. 1970. Pp. xxvii, 330. $12.00.

Alexander Dalrymple has long been one of the marginal figures in the history of British exploration and commerce in the East Indies and the Pacific, overshadowed by such imposing characters as Anson, Cook, and Vancouver. The late Vincent Harlow did something to rescue him from relative obscurity, in his *The Founding of the Second British Empire* (1952), comparing him with John Dee of Elizabethan times and citing his activities as part of the British "swing to the East." Professor Fry of James Cook University in Queensland now gives us this study of Dalrymple as both participant in, and propagandist for the enlargement of British commerce and influence, though not of territory, in the Indies and the Pacific.

Dalrymple has found a good advocate. Professor Fry has been assiduous in his search for sources and, for the most part, effective in his championing of his subject. But he has had his problems. For one thing despite his considerable publications, Dalrymple left neither a body of personal papers nor a diary. Lacking such sources, Fry is compelled to rely a good deal on presumption and conjecture about Dalrymple's role in the promoting of some of the British activities in the Pacific. There are a good many instances when "it is probable" or "it is inconceivable" have to be used by the author to buttress his case because of the lack of solid evidence. Further, Dalrymple was not a man given to compromise; he wanted action and the role of leader, but on his own terms. A man of somewhat less adamant and irascible disposition might well have had wider opportunities opened to him. Despite Fry's efforts it is still difficult to believe that the Board of Admiralty passed over Dalrymple in favor of Cook to command the famous voyage to Tahiti for the observance of the transit of Venus, simply because of navy regulations. Then there are the specific causes that Dalrymple advocated. A man who argued for the existence of the Great Southern Continent, for the commercial practicality of the Northwest Passage,

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and against settlement in Australia because it appeared to threaten the monopoly of the East India Company, presents difficulties to his biographer.

But the author's case is that out of such errors came truth. Dalrymple's advocacy of the Southern Continent contributed to Cook's famous voyages. His hypothesis of a commercial passage from the Atlantic to the Pacific south of the Arctic Circle aroused a British interest in the western coast of Canada that helped preserve much of the area for later British and Canadian settlement. And Dalrymple's more scholarly work achieved much that is worthy of notice. His collection and collation of accounts of past explorations, his dissemination of maritime knowledge, and more notably his work in establishing real British hydrographical skills all justify the author's interest in his subject.

DONALD C. GORDON
University of Maryland

R. M. HARTWELL, editor. *The Industrial Revolution*. New York: Barnes and Noble. 1970. Pp. x, 179. $7.25.

JOHN W. OSBORNE. *The Silent Revolution: The Industrial Revolution in England as a Source of Cultural Change*. New York: Charles Scribner's Sons. 1970. Pp. xi, 232. $7.95.

These books consider the Industrial Revolution from different points of view. One contains eight essays by past and present members of Nuffield College, Oxford, on various economic and social topics; the other describes the transformation that occurred in British values and institutions between 1780 and 1830 in response to industrialization. Despite their limitations, the precise, closely reasoned essays in Dr. Hartwell's volume break new ground; by comparison Professor Osborne's survey, based on familiar secondary sources published before 1966, is analytically superficial and not always reliable.

Two of the economic history essays in the Hartwell volume stress the importance for all-round economic growth of a progressive agriculture in Britain before 1750. According to Professor E. L. Jones the development of mixed farming after 1650 enabled British agricultural interests, unlike their West European counterparts, to meet the challenge of falling cereal prices. Professor D. Whitehead in an important paper on "The English Industrial Revolution as an Example of Growth" claims that "a fundamental turning point is unlikely to be a feature of evolutionary growth" and he rejects the view of Deane and Cole in *British Economic Growth* (1962) that the acceleration of output during the 1740s initiated an era of sustained growth; "for the first three quarters of the century . . . growth was taking place over a wide front and at more than a negligible rate." Stressing the creative role of businessmen, Dr. Hartwell challenges Sidney Pollard's central conclusion in *The Genesis of Modern Management* (1965) on the grounds that since the Industrial Revolution occurred, businessmen must have contributed significantly to this development by overcoming the increasing risks and uncertainties associated with factory production. In a detailed case study of the high pressure Cornish pumping engine, N. Von Tunzelmann illustrates an instance of technological diffusion in the early nineteenth century.

All this is excellent fare, more so in fact than the four essays on social history, which explore the consequences of industrialization in the early nineteenth century. Dr. M. E. Rose shows that opposition to the Poor Law Commission by Boards of Guardians in the West Riding lasted much longer and covered more issues than is usually supposed; and Dr. C. M. Elliott claims that conflicting attitudes between middle- and working-class Dissenters toward many social issues precluded common action and later produced a "nonconformist conscience" concerned with "relatively secondary issues." Although they explore significant group tensions, these contributions leave too many questions unanswered in view of the fact that both dissertations were based on local studies. Did northerners believe in the existence of a permanent pauper class? What were the political interests of the Guardians? To what extent would the inclusion of Methodism modify Elliott's findings? Were Churchmen expected to speak out with one voice on the great issues of the day? In an inconclusive pilot study based on parish registers, Dr. P. E. Razzell considers the mobility and the marriage pattern of laborers. Finally, Dr. Hartwell presents a masterly summary of

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"The Standard of Living Controversy," demolishing the three "growth plus immiseration" models of his opponents in the "catastrophic" (pessimist) school: "The facts of history," he triumphantly concludes, "conform with the expectations of economic theory," and he expects future controversy to focus on whether "a valued way of life" was replaced by "an inferior and degraded way of life."

In a lucid survey of the entire panorama of human endeavor, Professor Osborne says a great deal about changing styles of living. And with the rare optimism of an old-fashioned Whig, he believes that the Industrial Revolution suddenly improved the quality of life in England. The working classes were generally better off but, as they were unable to cope with the new conditions themselves, these submissive men and women depended upon assistance from the government. Thanks to the Industrial Revolution, however, England was half way on the road to becoming a humane democracy with a regulated welfare state by the 1830s. Unfortunately, Osborne's nostalgia for liberal England causes him to avoid controversial issues and his account is both unbalanced and at times inconsistent. The verdict that "with remarkably little friction England was able to alter itself so that what was clearly a revolution seems in retrospect evolutionary modification" rests upon certain views that will not go unchallenged. England in the mid-eighteenth century was not as the author infers by his comparisons like a present-day underdeveloped society. Even if the "shock waves" of the Industrial Revolution "were felt in almost every nook and cranny of English life," Methodism, efficiency, and humanitarian reform were not merely the products of industrialization. Far too much emphasis is placed on the degree of power and influence exerted by middle-class factory masters in the 1830s. Indeed many of the changes referred to by Osborne were brought about, as he shows, by minorities swimming against the tide. Until we develop more adequate tools for analyzing social change, surveys like this one that do not have a systematic framework are unlikely to attract more than passing interest.

GORDON RIMMER
University of New South Wales

FRANÇOIS VIGIER. *Change and Apathy: Liverpool and Manchester during the Industrial Revolution.* Cambridge, Mass.: M.I.T. Press. 1970. Pp. viii, 236. $12.50.

This title promises interesting observations, comparisons, and conclusions. Liverpool was an old corporation, returning members of Parliament, but Manchester until 1838 was only an unincorporated village, parish, and manor. Both had recourse to local acts for statutory *ad hoc* authorities; in Liverpool the chief of these being for the docks and in Manchester for the police commissioners.

Mr. Vigier is interested in the politics of these towns only insofar as they bear on his main interest: the failure of those governing them to avoid slums and eliminate poverty. "There is no doubt," he says, "that municipal authorities in Liverpool and Manchester were endowed with sufficient resources to have undertaken public works beneficial to the population at large, and particularly to the poor, without raising additional taxes." In this, for Liverpool, he has the support of Edwin Chadwick. Even after the changes of 1832–38 local government failed, largely because of a "lack of participation in public affairs on the part of the urban middle class," a surprising statement in view of, for instance, the civic activities of Unitarians.

The text is fortified by some excellent illustrations and by graphs and tables, some of them illuminating. Figure 1, "a hypothetical model of the planning process," suggests that the author has started with twentieth-century assumptions, which may have hindered his understanding of eighteenth-century processes. He believes that, in the communication between rulers and ruled, "the government . . . must isolate and identify relevant issues for community discussion and choice," a sentiment more congenial to Queen Elizabeth I than to luminaries admired in Manchester like Cobden, or to a Roscoe of Liverpool. In this spirit the commercial growth of Liverpool is described, *tout court*, as "the result of far-sighted policy by its municipal government."

There is in this book a good deal of general economic and constitutional history. Some anachronistic misunderstanding appears in the comment that in the Commons before 1832 400 seats represented "urban" places, but after

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1832, only 264. How "urban" were the places named in Schedule A of the act? Mr. Vigier seems unaware that the 1832 act preserved the voting right of resident freeman in those boroughs not in Schedule A, as life interests. To say that before 1832 urban interests "had no political voice in the country's economic affairs" is misleading and indicates that Mr. Vigier is not well acquainted with the present state of eighteenth- and early nineteenth-century studies. In economic and social history he seems to stand no better. His bibliography contains, except for Nicholas J. Frangopolo on Manchester (1962), nothing produced later than 1960, which is the more surprising in view of his major concern with demographical and social problems. This book is not a work of primary historical scholarship. There has been no recourse to any manuscript material, no use made of such business archives as might illuminate the manufacturing or commercial progress of either town, and too much reliance on survey books. Perhaps, the author intended it not so much as a work of academic scholarship as a tract for the times, troubled as they are by American urban tensions.

R. W. GREAVES
University of Kansas

D. J. MANNING. *The Mind of Jeremy Bentham.* (Monographs in Politics.) New York: Barnes and Noble. 1968. Pp. ix, 118. $4.50.

Dr. Manning's book is one of a series evidently aimed at providing trenchant but incisive summaries of leading political subjects. It does so. Often it is too trenchant. Chapters are frequently only two or three pages long. No specialist in the field is likely to think that proper attention has been given to all aspects of the subject: too much is left out. At the start, and indeed throughout, Manning presents Bentham as product of the urban anomic complex, the "society of strangers" that resulted from the break-up of traditional rural society. But this is only one aspect of the great change to which Utilitarianism and other ideologies responded around the beginning of the nineteenth century. There was a political and an intellectual as well as an industrial revolution. There was a society of equals as well as a society of strangers. And at the end of the book, Manning insists too dogmatically on the irrelevance of ideas to practical politics ("the utility of political philosophy to the practice of politics is nil.") This is a statement emanating from ideological conflict within contemporary Britain, roughly between Manning's master Michael Oakeshott and the Nuffield psephologists. In fact few more dramatic examples of the power of remote and abstract ideas to help shape political reality can be found than Benthamism. Manning is on sounder ground when he shows that Bentham's political schemes were only partly deducible from his theoretical principles, i.e. that he intruded a great deal of ideology, intuition, and prejudice into his allegedly purely rational system. And also in noting how guilty Bentham was of failing to recognize the elements of practical skill and craftsmanship in politics. In the three pages on educational ideas, the telling point is made against Bentham that while seeking a "relevant" education he thought in terms of processing the student, leaving out creative participation, a consequence of the deadening Lockean attempt to make a determinate scientific product of human minds. On the other hand the brief discussion of Utilitarian ethics seems especially inadequate in the light of the extensive recent philosophical literature, which Manning ignores. The bibliography is as brief and well selected as the book. In sum, there are many valuable insights in a book that cannot be considered in any sense definitive.

ROLAND N. STROMBERG
University of Wisconsin–Milwaukee

DANIEL A. MILLER. *Sir Joseph Yorke and Anglo-Dutch Relations, 1774–1780.* (Studies in European History, Number 23.) The Hague: Mouton. 1970. Pp. 131. 28 gls.

This slim book by Daniel A. Miller is designed to draw attention to the career of Sir Joseph Yorke and his efforts to maintain the neutrality of the Netherlands during the years of the American Revolution. Yorke served as British ambassador to the Netherlands from 1751 to 1780.

The disagreements between the Dutch and

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the British over the continued validity of the Anglo-Dutch Commercial Treaty of 1674 in the light of the British Rule of 1756 looms as a continuing point of difference in the maintenance of the Anglo-Dutch Defensive Alliance of 1678. The merchants of Amsterdam, always interested less in the point of view of Stadtholder William V than in their personal desire to carry on trade for the benefit of their own pockets, led directly to the argument between the British government and the Amsterdamers over the doctrine of "free ships, free goods."

Mr. Miller spends much time in stressing the pro-British attitude of the Stadtholder and his intimate adviser, the duke of Brunswick. In contrast to this view he clearly presents the pro-American viewpoint of the merchant aristocrats, particularly of Amsterdam, and the prodemocratic Dutch group known as the Capellan Patriots. This point of view arose not out of sympathy toward the American rebellion but because the revolt provided an opportunity for the Dutch merchants to make money by selling powder and military supplies, particularly at St. Eustatius, to the Americans. The case is clearly presented that Sir Joseph Yorke was the principal factor in preserving official Dutch neutrality until 1780. When his urging for a strong British stand failed to materialize, the Dutch finally, despite William V's pro-British attitude, joined the Armed Neutrality of the North, and the Anglo-Dutch Alliance of 1678 ended.

The book presents a clear and convincing argument for the important role of Yorke in maintaining the troubled neutrality of the Dutch in the face of the desire for profits and French pressure after 1778 to join in the war against England. The author incorporates some hitherto unpublished letters of Yorke to William Eden, British undersecretary of state. Otherwise the book offers little that is new on the correspondence of Yorke as ambassador or in interpretation of his efforts to maintain Dutch neutrality. The diplomatic history of Anglo-Dutch relations between 1774 and 1780 has been extensively investigated by other authors. The bibliography contained in this book is an excellent selection from both English and Dutch sources.

PAUL B. CARES
Allegheny College

L. G. MITCHELL. *Charles James Fox and the Disintegration of the Whig Party, 1782–1794*. (Oxford Historical Monographs.) New York: Oxford University Press. 1971. Pp. 318. $12.00.

It has been customary to condemn the political leadership of Charles James Fox. Historians point out that he led the Whig Party from defeat in 1783–84 to a disastrous split a decade later and that over a political life of thirty-seven years Fox held office for only eighteen months. His failure is ascribed to his indolence, his willfulness, his gambler's impetuosity, and his lack of judgment. This popular interpretation presents a problem that is usually evaded: how could so careless a politician maintain a life-long leadership over a party that included so many men of brains and ability?

In a well-written study that surveys all available evidence, Leslie Mitchell makes a convincing case for Fox's being a more astute political leader than many have credited. Fox emerges as a man of principle who threw away opportunities for office because he insisted that the House of Commons, not the king, should be the authority for ministerial advancement. Thus his break from Shelburne in 1782 was consistent with his opposition to the "corrupt influence" of the Crown under Lord North since Fox believed that Shelburne had become the agent of that corrupt power. Coalition with North was not an aberration, but rather exemplified Fox's belief that the chief issue in politics was the reduction of royal influence. The author suggests that Fox consciously set out to redirect the course of the Whig party when he advanced the proposition that a majority of the Commons could both unseat a government (Shelburne) and nominate its successor (Fox-North). The ensuing struggle with George III gave his party "a cohesion which it had never before enjoyed." Nor should Fox's opposition to Pitt be judged merely factious. Fox could never forgive Pitt's coming into office through the machinations of George III.

Mitchell argues that the crucial years 1782–84 provided the Whigs with "a common creed on which to act." During the following ten years Fox held his party together upon that issue, devoting many hours and much energy explaining, cajoling, and reconciling differences among his colleagues. Without Fox the Whig party would have likely dissolved. When

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Burke and the Portland wing of the party went over to Pitt, Fox was bound to choose to stay with Grey, Sheridan and Tierney if for no other reason than to keep the Whigs in existence as a party. Fox bequeathed a tradition of regard for civil and religious liberties, parliamentary reform, anti-slavery, and maintenance of party in politics. This last was not the least of his contributions.

ROBERT WORTHINGTON SMITH
University of Oregon

DALE H. PORTER. *The Abolition of the Slave Trade in England, 1784–1807*. [Hamden, Conn.:] Archon Books. 1970. Pp. xii, 162. $7.00.

Professor Porter's monograph promises more in its title than its author incorporates in his text. It is actually a study of the slave trade and the West Indian economy. His analysis of the vicissitudes of the sugar and slave business, the indebtedness of the planters, and the dramatic shift in the importance of the West Indies in the imperial economy, is a substantial contribution to the story of the final success of the movement for abolition. He argues persuasively that overproduction was the powerful incentive for Parliament's action in 1807. A particular strength of the book is the sections on the defenders of the slave trade and the case against abolition.

But it is perhaps a weakness of dissertations that they often pursue wasps with sledge-hammers and enhance their originality with attacks on "traditional" interpretations. The wasps in this case are the "Saints," and Wilberforce in particular, whose moralizing apparently disturbed the author. "I grew annoyed," the author tells us, "by his [Wilberforce's] overriding insistence, session after parliamentary session, on his own solution to the problem of the slave trade." The consequence of this annoyance has been a neglect of a public opinion that was created by Evangelical, Methodist, and Quaker conscience and that provided the important stage for 1807.

Clarkson, Wilberforce, and scores of others kept the fight against slavery alive despite the taint of Jacobinism. It would in no way have reduced the force of Porter's arguments had he given more credit to them. And Romilly is not mentioned in a book that examines the passing of the bill in detail.

I doubtless run the risk of a charge of pedantry by pointing out that there could not have been a Roman Catholic bishop of Liverpool in 1788 and that the term "Upper House" is not a suitable sobriquet for the House of Lords. And it is unfair to the duke of Portland (he who took the sting out of the repressive laws passed after 1794) to dub him a "reactionary." Nor should he be called "Colonial Secretary" although colonial affairs came within the purview of his home secretaryship.

GEORGE B. COOPER
Trinity College, Hartford

CARL WOODRING. *Politics in English Romantic Poetry*. Cambridge, Mass.: Harvard University Press. 1970. Pp. xvi, 385. $10.00.

The political ideas of the English Romantics, as Carl Woodring explains in his preface, have received ample attention from a number of distinguished scholars. What, then, is the reason for yet another volume on the topic? Woodring's explanation—and it is a very just one—is that even the standard work on the subject, Crane Brinton's, "relies on statements in prose more than on the configurations of diction and metaphor in the poems." It is, as "a professional student of poetry" that the author sets out to describe and analyze the political ideas of the period 1789–1832 in England. The question, inevitably, is how well he has done. Or, putting the matter another way, has the "professional student of poetry" done better than the professional students of politics who have written on the same theme?

On such a matter, opinions will differ, but this reviewer must admit to some disappointment with the results of this most recent effort to explore the political ideas of the Romantic poets. Granted, for example, that Woodring's chapter on Wordsworth is more fully documented, and in every sense more analytical of a greater number of Wordsworth's poems than is the very brief chapter by Alfred Cobban in his *Edmund Burke and the Revolt Against the Eighteenth Century*, does Woodring achieve the same level of insight into Wordsworth's politics that is present in Cobban's treatment? I would suggest that he does not. The sureness that Cobban showed in dealing with Wordsworth's nationalism was the product of his ready familiarity with political theory and political history. There is no substitute for either.

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There are significant differences of interpretation as between Cobban's analysis of Wordsworth's political sentiments after 1814 and those of Woodring. One cannot imagine Cobban writing the following: "More surprising than the movement toward order is his [Wordsworth's] continuance until at least 1835 as a poet of the underdog. One of his very late calls to be up and doing, the sonnet 'Highland Hut,' exhorts the reader to 'love, as Nature loves, the lonely Poor'" (p. 146). Cobban would have discovered no intellectual significance in such a line. Nor, for that matter, was he in any way "surprised" by what happened to Wordsworth's opinions after 1814.

The "student of politics" may indeed be handicapped when he approaches the poetry of men like Wordsworth, Byron, and Shelley. One wonders, however, whether his familiarity with political subjects will not always give him a certain immunity against the sort of enthusiasm that sees the Romantic poets as "students of power," and that exaggerates their political insight and influence.

STEPHEN R. GRAUBARD
Brown University

T. W. KEEBLE. *Commercial Relations between British Overseas Territories and South America, 1806–1914: An Introductory Essay.* (University of London Institute of Latin American Studies Monographs, Number 3.) [London:] University of London, Athlone Press for the Institute; distrib. by Oxford University Press, New York. 1970. Pp. vi, 108. $4.00.

This study, modestly subtitled "An Introductory Essay," was undertaken during a year's sabbatical leave from the British diplomatic service. Though relatively brief and based on printed materials, this lucid account sets relations between Britain and Latin America into a less familiar perspective. The conventional themes of metropolitan influences upon Brazil and Argentina are supplanted by an examination of the development of economic links between British colonies and South American countries. This shift of approach allows trade across the Pacific to receive an attention usually accorded only to that across the North Atlantic and in particular is instructive on the role of Chile in establishing shipping and supply links between Britain and Australasia. Although this trade represented a short-lived response to needs ranging from that of early nineteenth-century India for Chilean copper to those of the imperial forces in South Africa for Argentine foodstuffs during the Boer War, its successive forms demonstrated an indefatigable search for commercial opportunities in which merchants of British origin were everywhere prominent. Dr. Keeble clearly indicates the interlocking nature of British trading developments and points to one of the most effective grounds of predominance: the ports of South America were part of a system that did not need to distinguish between British and foreign control.

This survey deserves to be followed by more detailed and specific studies: as a general introduction it is deficient principally in its almost total neglect of relations between South America and the British colonies of the Caribbean. After 1806 there was a widespread belief that the Caribbean would provide a route of entry into the South American market, and some years passed before this hope was abandoned. Did this maintenance of separation between South America and the neighboring colonies demonstrate the failure or the flexibility of British commercial penetration? Dr. Keeble is silent on this point, but he is highly informative within a limited space on many other matters.

PETER MARSHALL
McGill University

D. B. SWINFEN. *Imperial Control of Colonial Legislation, 1813–1865: A Study of British Policy towards Colonial Legislative Powers.* New York: Oxford University Press. 1970. Pp. 202. $8.00.

After 1815 many British officials and politicians believed that it might be desirable to make some changes in colonial policy and to relax the bonds of imperial control. At the same time, Britain continued to expand her empire. The British government had no desire to allow her colonies—all of them in various stages of political development—to move too rapidly toward independence.

The Colonial Office and the Secretary of State possessed considerable authority over colonial legislatures. One important means of control over the colonies was the process of colo-

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nial law review. By this procedure the laws passed by the colonial legislatures could be ratified, amended, or disallowed by the Crown or have their constitutional validity judged by the courts.

To operate such a system was a formidable task. James Stephen, legal adviser to the Colonial Office, estimated that he had been required to report his opinion on about twenty-one thousand colonial laws between 1815 and 1846.

D. B. Swinfen's book is divided into four parts dealing with the review process; imperial law and colonial laws; imperial policy and colonial laws; the Colonial Office and the Colonial Laws Validity Act. In appendix A is the text of the Colonial Laws Validity Act, appendix B contains the statistical tables of colonial acts, 1813–65, and appendix C lists the colonies whose acts passed through the Privy Council from 1813 to 1865.

In this excellent monograph Swinfen carefully explains how the process of colonial law review actually worked. He also tells us a great deal about general trends and principles and the administrative organization of the Colonial Office. With excellent illustrations and superb analysis, he shows who was in fact responsible for shaping a large part of colonial policy.

Of special interest to students of imperial history are the facts and comments about James Stephen and Sir Frederic Rogers, those shrewd and tremendously able legal advisers. Of particular value, too, is the careful examination of the slowly changing attitudes of the British government to the legal relationships of Britain and her colonies in the years before the passage of the Colonial Laws Validity Act of 1865 that re-defined the powers of the colonial legislatures.

Swinfen's sound and scholarly work will be read with interest and profit by all scholars interested in the growth of British Colonial Office policies and procedures between 1813 and 1865.

GOLDWIN SMITH
Wayne State University

D. P. O'BRIEN. *J. R. McCulloch: A Study in Classical Economics.* New York: Barnes and Noble. 1970. Pp. 452. $16.50.

J. R. McCulloch had a bad press while he lived—he was Peacock's MacQuedy and Carlyle's M'Croudy. Nor has posterity been more generous. D. P. O'Brien's work, the first thoroughgoing study of McCulloch to appear, attempts to remedy this unfavorable view of the man long regarded as the word of the "dismal science" made flesh. This is a scholarly, conscientious compilation of McCulloch's views upon almost every aspect of political economy, which draws upon virtually everything that this extraordinarily prolific journalist wrote. The book should prove a considerable boon for specialists in the history of economic thought.

The non-specialist reader, whether economist or historian, is probably less well served because the author is reluctant to part with any fact that has come his way: many of these are to be found in footnotes, which occupy more space than the text. Even specialist readers may feel that O'Brien has waged too much of an up-hill fight to prove that McCulloch was not merely an undeviating disciple of Ricardo, which has been more or less the common view. The author tends to exaggerate McCulloch's departures from Ricardian doctrine, as when he describes his subject's espousal of what was at most a widely shared modification of Ricardo's theory of value as a *volte-face*. O'Brien sees McCulloch as "very firmly in the Scottish tradition" of Adam Smith, but beyond the statement that the Scots were interested in aligning fact and theory and that McCulloch was a statistician the author does not describe this tradition, its other adherents, or how McCulloch fits into it. Almost every nineteenth-century economist, whatever his views, professed, and with some reason, strong links to Smith. But if a "Smithian tradition" of political economy were to be constructed as a counter to the Ricardian, there is more to be said for Malthus's well-argued view that it would include himself and Chalmers, rather than McCulloch.

In an address to the Commons in 1845, which the book does not quote, Richard Cobden, chaffing at McCulloch's role as the adviser to the Tory government, described McCulloch as "a painstaking statistician" who had not contributed "a single new idea" to the science of political economy. O'Brien in his effort to rehabilitate McCulloch has certainly demonstrated that McCulloch was not merely Ricardo's parrot, and no doubt McCulloch did initiate minor advances in economic theory. But even

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when compared with such distinctly secondary figures as Senior or Torrens, McCulloch does not measure up and Cobden's jibe rings true.

BERNARD SEMMEL
State University of New York, Stony Brook

JUDITH RYDER and HAROLD SILVER. *Modern English Society: History and Structure 1850–1970.* London: Methuen and Company; distrib. by Barnes and Noble, New York. 1970. Pp. viii, 340. Cloth $9.50, paper $4.50.

This book by two authors turns out to be in reality two books bound together. It is an attempt to define the modern English social structure in historical terms—society as it is now and how it got that way. Such an ambition is unimpeachable, but whether it could ever be achieved, even in a volume many times the size of this one, is another question.

In any case the authors are held back by the unnecessarily rigid framework in which they work. The first part of the book is essentially a history of social institutions—the welfare services, education, the rise of industry. The concluding sections are a survey of contemporary British society, bearing little relation to what has gone before. Much of the latter part reads like a summary of Anthony Sampson. Inevitably the book is fragmented. Social history is not quite the same thing as sociology even if one attempts to write it in sociological terminology. The descriptive analysis at the end of the book, divorced as it is from the earlier chapters, provides no useful conclusion.

On the other hand, by surveying the vast recent literature on the British social structure the authors have performed a service. They argue and document the paradox of an increasingly affluent working class that remains somehow still a "lower" class. Their discussions of the continuing influence of the Public Schools on university education and of the institutionalization of the class structure built into the Butler Education Act of 1944 are excellent. Snobbery remains the English religion, and the class structure is its church. As a summary of recent statements of this not particularly novel thesis the book has substantial merit.

BENTLEY B. GILBERT
University of Illinois, Chicago Circle

JOSEF L. ALTHOLZ and DAMIAN MCELRATH, editors. *The Correspondence of Lord Acton and Richard Simpson.* Volume 1. New York: Cambridge University Press. 1971. Pp. xxvi, 228. $16.00.

DAMIAN MCELRATH, in collaboration with JAMES HOLLAND *et al. Lord Acton: The Decisive Decade, 1864–1874. Essays and Documents.* (Bibliothèque de la Revue d'histoire ecclésiastique, Number 51.) Louvain: Publications Universitaires de Louvain; distrib. by Éditions Nauwelaerts, Louvain. 1970. Pp. xix, 275. 400 fr B.

The posthumous career of Lord Acton is following the usual pattern: in the years following his death a spurt of publications consisting of the essays and lectures that he had conspicuously failed to publish during his lifetime and of the letters that had evidently taken the place of more sustained work; a few decades of almost complete neglect; the appearance, a half century after his death, of the first full-length biographies and commentaries; and within the past several years, books on particular aspects of his life as well as several multivolumed collections of his correspondence. As one who once had occasion to deplore the paucity of material by and on Acton, I must now confess to feeling somewhat uneasy about what threatens to become a major academic industry.

Part of my uneasiness comes from the form of some of the recent publications. Instead of the single corpus of collected correspondence that the scholar might expect, or the selected correspondence that the ordinary reader might prefer, we are being presented with separate collections of letters to different correspondents. Victor Conzemius has published the first two of three volumes of the Acton-Döllinger correspondence, Josef Altholz and Damian McElrath have just issued the first of three Acton-Simpson volumes; and Acton-Gladstone series is contemplated; and others are vaguely spoken of—all this supplementing the older three volumes of correspondence. Each of these collections, moreover, covers a substantial period of Acton's life. Anyone interested, for example, in what McElrath calls "the decisive decade" (1864–74) or in a single event (such as the Vatican Council) would have to consult them all—in effect compiling his own collected correspondence.

Another source of uneasiness is the tendency of some of the editors to make excessive claims for their material. The contemplated three vol-

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umes of Acton-Simpson letters (of which this first volume covers eighteen months of 1858–59) can be justified by the simple and old-fashioned criteria of definitiveness and comprehensiveness—that is, publishing everything about an important person or event that a publisher can be persuaded to put into print. Acton was such a person, and the Liberal Catholic movement in England was such an event. It is, however, neither necessary nor plausible to suppose that Simpson was the "brilliant and scientific" Shakespearean scholar he is here made out to be; pretensions of this sort make suspect what is otherwise an unexceptionable enterprise, indeed a model of patient and meticulous scholarship.

Similarly, the "decisive decade" was just that in Acton's life and in the history of nineteenth-century Catholicism. But the volume of that title does not seriously attempt "to unravel the enigma of Lord Acton's commitment to moral judgment in history." The enigma, in any case, lies not in his commitment to moral judgment, but in some of his actions and convictions in the light of that commitment. And here we find little that is new. Nothing new, for example, to account for his acquiescence in 1874 to the decrees of infallibility, an acquiescence expressed in terms very similar to those that he had so sharply denounced in 1871. (This is not to suggest that his acquiescence was in any way dishonorable or sophistical, but only that it was at variance with his earlier position.)

The Decisive Decade is an interesting volume, not so much for what it tells us that is substantively new, but for what it implicitly tells us about the new source material with which we are now being presented. The volume consists of essays and supporting documents on several aspects of Acton's life during that decade. Yet in spite of an entirely proper attempt to use, wherever possible, documents that had not previously been published, the good judgment and integrity of the editors have produced a volume in which only little more than half of the documents are entirely new. And neither the documents nor the interpretive essays support the claim that the new sources "added substantially to our knowledge" of Acton or "appreciably supplement or correct broad areas of Actonian studies." One of the few cited instances of "correction" is based upon a misreading of the original text; here, as elsewhere, what the documents and essays generally do is to confirm what was previously known. And one of the longest and most interesting of the new documents, "Notes on Archival Researches 1864–68," is an address delivered in 1895 or "sometime thereafter." To treat this, as the editors do, as if it were a contemporary document faithfully reflecting Acton's activities and attitudes in those formative years is to miss a splendid opportunity of comparing his retrospective judgments with the views that he actually expressed in his correspondence at the time.

It would be unfortunate if unwarranted claims to novelty or occasional editorial lapses were to detract from the worthiness of this and similar publications. Collections of primary sources are as important for purposes of confirmation and elaboration as for correction or innovation. In 1895 reviewing his archival researches of earlier years, Acton concluded: "To renounce the pains and penalties of exhaustive research is to remain a victim to ill informed and designing writers, and to authorities that have worked for ages to build up the vast tradition of conventional mendacity." "By going on from book to manuscript and from library to archive, we exhange doubt for certainty, and become our own masters. We explore a new heaven and a new earth, and at each step forward, the world moves with us."

The last sentence is a rhetorical (or perhaps not entirely rhetorical) flourish that most historians would shy away from. And few of us have the problem of exposing a "vast tradition of conventional mendacity." But what all of us have is the need to "exchange doubt for certainty." This is the sufficient justification, and the considerable value, of publications such as these.

GERTRUDE HIMMELFARB
City University of New York

ROBERT M. YOUNG *et al. The Victorian Crisis of Faith: Six Lectures.* Edited by ANTHONY SYMONDSON. London: S.P.C.K., in association with the Victorian Society. 1970. Pp. 126. £1.75.

M. A. CROWTHER. *Church Embattled: Religious Controversy in Mid-Victorian England.* (Library of Politics and Society.) [Hamden, Conn.:] Archon Books. 1970. Pp. 272. $9.25.

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Oddly but interestingly the six lectures on *The Victorian Crisis of Faith,* arranged originally by the Victorian Society for delivery at the National Portrait Gallery in London, reduce the commonly assumed dimensions of that crisis. Canon Max Warren has to invest the word with potentialities for progress and advance before he can fit the missionary movement under the general title. There is no sense of crisis at all in Canon R. C. D. Jasper's account of the slow modification of the Prayer Book in the nineteenth century. Though Dr. Robert Young, in his discussion of the impact of Darwin, confesses his own belief in the ultimate incompatibility of science and transcendental religion, he emphasizes both the unwillingness of the main evolutionists to attack religion and the attempt by Christian and unorthodox thinkers of the period to resolve the conflict. Dr. David Newsome has no difficulty relating his subject of John Henry Newman and the Oxford Movement to the general title; yet he points out that Newman was anxious to accentuate the sharpness of the conflict between Anglican Catholicism and the prevailing spirit of accommodating, reconciling liberalism. In Newman's estimation, too few Victorians appreciated the urgency of the crisis.

Two of the lectures stand out. Professor Geoffrey Best's examination of the Evangelicals in the Church of England has a density and freshness that could make it a small classic. He draws attention to the infusion of new enterprise into the ministrations of the Church by Evangelicals even before William Wilberforce. While not ignoring their later spiritual enfeeblement, Best claims for them an overwhelming primacy among Victorian religious groups in philanthropic work, and he exonerates their philanthropy from the charge of political conservatism. He also points out the deep comfort that Evangelical religion gave to many oppressed by the mystery of death, however crude that religion might seem to more refined tastes. Professor Owen Chadwick's lecture on "The Established Church under Attack" reveals the process of inquiring scrutiny by a distinguished mind into the nature of the assault and its degree of success by the close of the century. His examination makes it abundantly clear that the information we have with which to answer the question is meager and ambiguous, and that a great deal of detailed research—Chadwick presses for studies of local religious history—will have to be conducted before generalizations can be hazarded with anything better than stammering diffidence.

The carefulness of Chadwick's argument is in sharp contrast to the flaccid generalizations of Dr. M. A. Crowther's *Church Embattled.* They often fall just short of outright error and are compounded with confused labeling of the schools of thought within the Church of England. The effect is almost to obliterate the kernel of the work, a study of the thought of three mid-Victorian Broad Church clergymen—Rowland Williams, H. B. Wilson, and Charles Voysey. The first two were the only writers in *Essays and Reviews* who were prosecuted before the courts for their contributions, as it turned out unsuccessfully. Voysey's later condemnation by the Judicial Committee of the Privy Council, in 1871, provided the first assurance that the Church of England's subjection to the civil courts would not totally eviscerate the body of theology to which it was committed. Dr. Crowther's useful analysis of the Broad Church mind is further enveloped in a diffuse argument that the Church's internal theological disputes, aggravated by the Broad Church attempt to expand the range of inquiry, impeded the development of centralized organizaton and direction within the Church. So far the thesis is unexceptionable, but in going on to contend that the failure to achieve unified organization was responsible in large part for the frustration of the Church of England's hopes, Dr. Crowther accepts a shallow estimate of the problems confronting the Church and reveals a naive faith in the efficacy of central government.

PETER T. MARSH
Syracuse University

ELIZABETH ISICHEI. *Victorian Quakers.* (Oxford Historical Monographs.) New York: Oxford University Press. 1970. Pp. xxvi, 326. $9.00.

This is excellent denominational history. Mrs. Isichei, who is not herself a Friend, has written a book that embodies some of the more attractive Quakerly virtues. It is a straightforward, plain-spoken account of the Society of Friends in nineteenth-century England. It is sympa-

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thetic, yet detached, and there are no symptoms of hagiography.

Mrs. Isichei is at her best when she deals with the internal history of the Society. Her description of theological change is full and impressively lucid. At the beginning of the nineteenth century Quaker theology was still suffused with quietism and the inner light. The theology of withdrawal was reflected in the peculiarities of Quaker dress and Quaker speech. By the middle decades of the century quietism was overwhelmed by evangelicalism. Withdrawal was transformed into struggle, and peculiarities were de-emphasized. After 1860 marriage with a non-Friend no longer meant automatic expulsion from the Society. This evangelical phase was the great age of Quaker philanthropy. It was also the period in which the Society was smallest and socially most cohesive. Toward the end of the century liberal modernism became a powerful theological current that further swept away distinctive sectarian behavior.

The thorough descriptions of formal schisms are well integrated into the discussion of theological change, and so is the craftsmanlike account of the formal organization of the denomination. Even more interesting is the skillfully drawn picture of the Society as a closely knit community of upper-middle-class families, a community bound together by genealogy and family traditions as much as by theological commitment. The author draws from the centrally kept register of deaths a few samples of occupational structure that help illustrate the social composition of the Society. She also examines the social character of meetings in Norwich and Manchester. These local studies raise such intriguing questions that one wishes there had been room for more than two. Why did the upper-middle-class domination of the meeting in Norwich decline during the latter half of the nineteenth century while it did not in Manchester? Which of the two cases is the more typical of provincial Quakerism? Mrs. Isichei, however, is principally concerned with those great families whose importance was more on the national level. Much like Unitarians, Victorian Quakers considered themselves a spiritual elite, the aristocracy of dissent. Concentrating her attention on the most aristocratic among them, Mrs. Isichei describes with sensitive perception a style of spiritual and social life that seems more Victorian than Quaker. It is a style that differs only in detail from that of those fathers of the Victorians, the Evangelicals of Clapham.

The role Victorian Quakers played in politics, moreover, differed little from the role of Protestant dissenters generally. They produced few significant political leaders, and they supported a variety of liberal causes including, usually, the Liberal party. Nor is it clear that in philanthropy Friends differed significantly from other evangelicals. Quakers were notably generous, but they may well have seemed particularly philanthropic because they were particularly rich. Only one of the many philanthropic causes that attracted Quakers was peculiar to the denomination—the adult Sunday school movement. Why adult education should have been particularly appealing to Friends is difficult to understand. Perhaps, as Mrs. Isichei suggests, the accidents of whim and fashion are the best explanations.

While the limitations of denominational history, some of which have been suggested here, should be recognized, they do not, of course, invalidate denominational history as a historical form. Within those limitations Mrs. Isichei has written a very good book indeed. *Victorian Quakers* should add appreciably to the reputation of its genre.

R. J. HELMSTADTER
University of Toronto

CHRISTINE BOLT. *Victorian Attitudes to Race.* (Studies in Social History.) Toronto: University of Toronto Press. 1971. Pp. xviii, 254. $10.50.

Victorian attitudes to race were of the armchair variety. Few black or colored exslaves, seamen, or petitioners from the Empire made their way to the British Isles. Prejudices were formed at second hand, from accounts of plantation owners, travelers, military men, and missionaries; and they were given a quasi-scientific basis by the ethnological and anthropological societies.

Christine Bolt analyzes these attitudes by focusing on the British response to the reconstruction of the American South, the Jamaican uprising of 1865, the conquest of Africa, and the vicissitudes of the Indian Empire. We are re-

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minded that the humanists' pity for the natives often harbored contempt, that missionary zeal was usually a thinly disguised form of racial and cultural arrogance, and that fear and anxiety lay at the base of Victorian racial pride. The imperialist, rightly seeing himself as an intruder having to cope with a foreign climate and indecipherable languages, religions, and folkways, knew that he was in a numerical minority. Native strength, if organized and equipped, could have unseated white power. Thus even the most racist of statements from the Empire contained "a clear undercurrent of *fear, disguised by bravado.*"

The issue of race was closely associated with the question of power. Anglo-Saxon imperial supremacy proved, to the satisfaction of many Victorians, the innate superiority of the white man. On the other hand, Britons consistently admired the strength of the more vigorous African tribes and reluctantly praised the higher castes in India—those nonwhites displaying the qualities of independence and fortitude that were supposedly characteristic of Aryans themselves. Power and authority were the crude rules of the game of racial distinction in the age of Darwinism. Yet color itself was important. Rationalizations of suppression and discrimination were built, in the final analysis, upon the color difference. The uncritical assumption of "white over black," so embedded in traditional Western European thought, was still operative in the Victorian era.

A treatment of this subject being long overdue, Dr. Bolt's work commands our attention. She effectively uses not only the pertinent monographs and newspapers but also the journals of the various scientific, philanthropic, and colonial societies. Yet in several respects this book is disappointing. One often loses the theme amid the details of the American, Jamaican, African, or Indian scenes. More important, one is not put into touch with that large body of popular racial opinion—exemplified in Charles Kingsley's Anglo-Saxonism—that had little direct reference to the Empire. And the title itself is misleading. Although there is merit in avoiding a superficial survey of the entire century, our attention here is largely limited to a period of about twenty years, from 1860 to 1880. We would like to know more about the racism in the South African question around 1900—for example, Cecil Rhodes's grotesque racist views. We especially need to know of the racial attitudes in Britain during the first half of the nineteenth century. Otherwise we are at a loss to interpret the enormous popularity of *Uncle Tom's Cabin* in England, or to ascertain whether the Jamaican revolt merely revived rather than increased that "vulgar tendency to see all black men as alike and inferior."

WILLIAM J. BAKER
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F. M. LEVENTHAL. *Respectable Radical: George Howell and Victorian Working Class Politics.* Cambridge, Mass.: Harvard University Press. 1971. Pp. xv, 276. $7.75.

The mid-Victorian phase of English working-class politics, a relatively quiet interlude between Chartism and socialism, has been neglected by historians. This is a pity, not only because of the resulting gaps in our knowledge but also because it fosters a neo-Whig view of modern English history, one in which popular militancy is the chief motor of steady progress toward socialism. Having witnessed a quarter century of welfare capitalism, however, we may now be more inclined to treat mid-Victorianism not as an aberration but as a paradigm: after G. J. Harney comes George Howell, and after Keir Hardie, Ramsay MacDonald. Lenin's comments on "trade-union consciousness," framed with an eye on Victorian England, make a good deal of sense.

Under these circumstances F. M. Leventhal's fine study of George Howell, the most important figure in English working-class politics between 1865 and 1875, is especially welcome. Drawing on the rich materials assembled in the Howell collection at the Bishopsgate Institute, Leventhal has written a definitive political biography, informed by a steady and dispassionate intelligence. We can follow in detail the career of a man who exemplified trade-union consciousness and Lib-Labism. First as secretary of the Reform League and then as secretary of the parliamentary committee of the Trades Union Congress, Howell was deeply involved in the agitations for extension of the franchise and reform of the labor laws. From this vantage point Leventhal guides the reader

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through the maze of working-class politics. He sets forth clearly the ideological and psychological foundations of Howell's political behavior. An excellent first chapter, "The Making of an Artisan," in which the author shows the formative influence of Wesleyanism and the artisan ethic, provides the backdrop for what follows. Throughout the book Leventhal writes with the quiet authority that comes from a thorough mastery of his materials.

Howell is not particularly appealing as a person. Intent on gaining the good opinion of his social superiors, he represents in rather extreme form the sort of *embourgeoisement* to which a segment of the working-class elite succumbed. Leventhal makes no attempt to minimize this aspect of his protagonist's character. As the title implies, one of his main themes is Howell's preoccupation with "the road to bourgeois respectability." Yet he handles this awkward matter with a very sure touch. The picture that emerges is properly ambiguous. Howell never wavered in his devotion to the substance of working-class interests as defined by the prevailing liberal consensus, but his choice of tactical forms occasionally brought him rather close to a sellout. We are ready to "understand" him sympathetically because he was so much the product of his age. But it is also important to keep in mind that other working-class leaders did not feel obliged to walk quite so far down the road to accommodation with the Liberal politicians.

This book is dedicated to the memory of David Owen. It is a fitting tribute.

TRYGVE R. THOLFSEN
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BENTLEY B. GILBERT. *British Social Policy, 1914–1939.* Ithaca: Cornell University Press. 1970. Pp. viii, 343. $9.00.

FRANK HONIGSBAUM. *The Struggle for the Ministry of Health, 1914–1919.* (Occasional Papers on Social Administration, Number 37.) London: G. Bell and Sons. 1970. Pp. 80. £1.50.

This is the second of a projected three-volume study of the development of social policy. As Gilbert points out, the present work is not precisely a continuation of his *Evolution of National Insurance: The Origins of the Welfare State* (1967) but is concerned more with the search for a new "political consensus" on social policy.

Gilbert asserts that for "most of two decades between the wars Britain had no social policy and was in search of agreement on the degree of State responsibility toward individual citizens" (p. 305). Yet as he himself repeatedly points out when he discusses specific legislation, enlightened British leaders realized soon after the First World War (as had Lloyd George earlier) that the extension of the franchise and the lure of the Labor party demanded that the state accept new responsibilities toward its citizens. By the end of 1921 "the rulers themselves," though they refused to admit it, accepted the workers' insistence on "the right to work or maintenance" (p. 86). The British ruling class always yielded to the inevitable. By 1939, despite the singular and huge handicap of massive unemployment, British leaders arrived at an "understanding whether or not publicly accepted" that a national minimum standard of living had to be guaranteed to British citizens (p. vii).

To find this "remarkable" is to overlook much of English history. The failure to enunciate a social policy did not mean that there was no social reform. If it came slowly and belatedly, that is the history of much legislation, especially in England. There were, of course, "economic cretins" who longed for a return to Gladstone's platform of proclaimed retrenchment and who were aghast at public expenditure (which involved taxation) because, as Baldwin phrased it in a memorandum to the Bonar Law cabinet in 1922: "Money taken for Government purposes is money taken from trade and borrowing" and would "depress trade and increase unemployment." Yet there was no turning back, except for moments when an attempt was made to carry out enunciated principles. Principles might have been asserted, but they rang hollow when practicing politicians feared that they might become dead statesmen. Even Neville Chamberlain, who lacked any sense of social vision, used his great talents to improve administration rather than to attempt to abandon welfare benefits. Only to force the Labor government out of office in 1931 did he demand a cut in unemployment benefits.

Surely politicians and bankers argued over

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symbols, as Gilbert so well puts it. The "ultimatum" that forced the resignation of the Labor government in 1931 was over a relatively small cut in unemployment insurance that was demanded by the American bankers and the Conservatives. Unemployment insurance was seen as the epitome of government extravagance, while the Trades Union Congress resisted the cut even at the cost of the dissolution of the Labor government. Bentley's narrative and his analysis of this "crisis" are unexcelled.

If Gilbert fails, in my opinion, to support his thesis, his individual chapters (on social politics, as he phrases it, rather than social policy) are informative, and his approach is often fresh and provocative. His characterizations of the leading figures are generally fair, even when his sympathies are elsewhere. He has used both primary and secondary material with discrimination (as is also evidenced by his first volume), and his bibliographical comments are acute. He may contend that social politics "usually emerge when the consensus on social policy breaks down," but when was there a widely accepted and articulated consensus on social policy? Even today the prime minister, though admittedly a romantic (Utopian?), is seeking to have the government role revert to what was idealized (if not quite practiced) as mid-Victorian liberalism.

Honigsbaum's pamphlet describes in detail the establishment of the Ministry of Health in 1919. It is part of a larger study in progress in general medical practice in Great Britain. Gilbert states both in this volume and the one preceding it that Lloyd George was genuinely concerned with the nation's health but asserts in the first volume that Lloyd George introduced the national health insurance act of 1911 because he wanted "to replace income not to cure sickness." Honigsbaum points out, on the specific evidence of Sir George Newman, that Lloyd George was indeed trying to "cure sickness," though political pressures forced many compromises.

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THOMAS JONES. *Whitehall Diary*. Volume 1, *1916–1925;* Volume 2, *1926–1930*. Edited by KEITH MIDDLEMAS. New York: Oxford University Press. 1969. Pp. xxiv, 358; xiii, 311. $40.00 the set.

These first two volumes of Thomas Jones's *Whitehall Diary* make fascinating reading and are indispensable to anyone studying any aspect of English political history in the decade and a half from 1916 to 1930. Brought into the civil service by a fellow-Welshman, David Lloyd George, who found him so useful in preparing material for speeches, Jones was appointed as one of the assistant secretaries of the War Cabinet in December 1916. Subsequently he became deputy secretary to the cabinet under Sir Maurice Hankey, acting as "liaison officer with the Departments which dealt with industrial questions and secretary to many Cabinet Committees." As he himself stated: "There were many opportunities before and after meetings, for brief and hurried conversation with Ministers and one learned to make good use of these occasions to judge imponderables and garner impressions of value to the Prime Minister, to whom, from the beginning, I had the privilege of direct access whenever I sought it." To Lloyd George's successors, Bonar Law, Stanley Baldwin, and Ramsay MacDonald, he was equally invaluable. A Welsh radical Nonconformist, describing himself as a "civil servant, law abiding, a believer in ordered progress," he wrote that he had "no credal difficulty in serving each Minister in turn; we were all infected with Liberalism."

The *Diary,* which also includes letters written to Jones and by Jones, furnishes a running commentary on the significant questions that concerned the government. Jones had true historical insight as he wrote, using not only the minutes of the cabinet and cabinet committee meetings he attended and recorded, but also the materials he received from the departments and civil servants to be funneled into drafts of speeches.

His acuteness in estimating people was apparently invaluable. When Baldwin in 1929 was planning his prospective cabinet, for example, he asked Jones about putting Churchill at the India Office. Jones replied: "A splendid notion, as Winston at the Irish job had shown that the bigger the post the better he is. . . . The important thing was to see that he had around him officials who were not afraid to stand up to him at critical moments when he was going off the deep end." Jones's perceptive comments make the participants in these years come alive as human beings with their

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strengths and weaknesses. One becomes aware of how decisions were really made.

Keith Middlemas has done a generally excellent job in editing. In his introduction he explains his guidelines in cutting the original of over 240 bound volumes in the National Library of Wales at Aberystwith. Each year is prefaced with a concise summary of the important events. Necessary explanations of allusions and elisions are inserted into the text, and the people referred to are identified in footnotes and appendixes.

MADELINE R. ROBINTON
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MARY MCNEILL. *Vere Foster, 1819–1900: An Irish Benefactor.* (Publication of the Institute of Irish Studies, The Queen's University, Belfast.) Newton Abbot: David and Charles. 1971. Pp. 259. £2.75.

One of the pleasant things about the field of Irish history is that there is still room for the contribution of the gifted amateur. Miss McNeill's biography of Vere Foster is a case in point. Anyone who serves in these times, as does Miss McNeill, as a member of Belfast's juvenile court deserves respect; anyone who does so and simultaneously produces a well-researched biography deserves admiration.

This book's subtitle, "An Irish Benefactor" is somewhat misleading, hinting as it does of pious good works. Instead, Vere Foster's actions were those of a benevolent buccaneer who fought social evils wherever he found them. Foster was the great-grandson of "the Earl-Bishop," Frederick Augustus Hervey, and the willful, but always attractive, eccentricity of that forebear typified Foster throughout his own life. Neither Eton nor Christ Church, Oxford, domesticated young Foster, and when, after spells in the home and the foreign services, he visited Ireland for the first time in 1847 his energies and sympathies were immediately engaged. He took a year's course of agricultural training at the Glasnevin Model Farm and then initiated a scheme of emigration from rural Ireland to America. Several times Foster himself led emigrant bands on the torturous voyages to the United States. Then in 1858 he turned to matters of popular education—building schools, supplying school requisites, editing an excellent series of copy books, and becoming the president of Ireland's first successful national teachers union. In his later years he produced a notable book of family history, *The Two Duchesses,* detailing the relationship between Lady Elizabeth Foster and Georgiana, duchess of Devonshire.

There are a number of minor factual errors in background material. These do not, however, detract from the clarity of the portrait of Foster, and the professional historian easily can make the necessary corrections for himself.

DONALD H. AKENSON
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NELLY GIRARD D'ALBISSIN. *Genèse de la frontière franco-belge: Les variations des limites septentrionales de la France de 1659 à 1789.* (Bibliothèque de la Société d'Histoire du Droit des Pays Flamands, Picards et Wallons, Number 26.) Paris: Éditions A. & J. Picard. 1970. Pp. 434, 3 maps. 125 fr.

This account of the evolution of the Franco-Belgian border in the *ancien régime* is distinguished by careful scholarship and ample documentation. The author has done extensive archival research in Paris, Lille, and Brussels, as well as in the library of the Prince de Ligne at Beloeil, and is fully in command of the pertinent literature in the field. In successive steps she examines each of the major treaties during the reign of Louis XIV and in the eighteenth century as it bears on the frontier question and the delimiting of jurisdictions. The greatest value of this book is the intensive study of the conferences after each treaty was signed. It was at these conferences that the delegates attempted to determine precisely what the new circumscriptions would be. Dr. Girard d'Albissin here displays the fruit of her work in the archives to best advantage. She provides pen portraits of the principals in each session; the duel of wits between the French and Spanish negotiators in Louis XIV's reign is especially well handled. The intentions of the opposite parties are explicated with the aid of instructions to ambassadors, interdepartmental notes, dispatches from the delegates at the conferences, and, of course, maps that illustrate her arguments quite well. Thanks to her organization and the accompanying maps one can grasp the opposing strategies at each peace conference and relate them to the ensuring military campaigns. Though military needs were unquestionably the chief consideration, eco-

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nomic factors and the tightening of customs that began during Colbert's ministry are also accorded their due importance. A particularly interesting section deals with the effect of boundary changes on ecclesiastical jurisdictions, changes in some instances from Spanish to French Catholicism.

The development of a more linear frontier from the sea to the Meuse was the product of one century of war and two centuries of negotiations. The author does justice to the effect this had on the economy, the society, and the daily lives of the people in the border territories. The maps are more than adequate and the index quite serviceable.

JOHN T. O'CONNOR
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BERTRAM EUGENE SCHWARZBACH. *Voltaire's Old Testament Criticism.* (Études de philosophie et d'histoire, Number 20.) Geneva: Librairie Droz. 1971. Pp. 273.

This is a most welcome complement to the recent re-edition of Professor Pomeau's study of Voltaire's religion. Professor Schwarzbach (who has had a closer look at the latest Old Testament scholarship than Pomeau), makes a convincing case for his thesis that "the Voltairean legacy in Bible studies is one of intelligent inquiry, wide curiosity and ingenious guesses."

Voltaire seems to have half-guessed that the effort begun in the late seventeenth century by Spinoza, Simon, and others to apply the new scientific method to the study of the Scriptures was based on a rather questionable premise—that religious and scientific truth were subject to the same norms and would yield similarly verifiable results. Yet, beginning in the 1760s (Schwarzbach dismisses earlier efforts as unimportant), Voltaire diligently applied the critical techniques inherited from an earlier generation to the Old Testament text. The results, as we are shown, are better than those achieved by Bossuet and quite as impressive as those attained by most of Voltaire's contemporaries, orthodox or heterodox.

Much of what Voltaire wrote was deist polemic, designed to undermine the credibility of the particular Jewish revelation. Yet he could be genuinely moved by his text, as he was by Ecclesiastes. Caught somewhere between Locke's marginal orthodoxy and Hume's detachment, not quite able to wrestle free of the elaborate coil of baroque apologetics in which he had become entangled, Voltaire emerges from this study as a more serious and perceptive student of Holy Writ than either critics or detractors have surmised.

GEOFFREY ADAMS
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E. TANGYE LEAN. *The Napoleonists: A Study in Political Disaffection, 1760–1960.* New York: Oxford University Press. 1970. Pp. xiv, 402. $12.75.

This is the second study by E. Tangye Lean of disaffection in the nation-state. The first, *Voices of Darkness* (1943), was concerned with the techniques of propaganda used to promote disloyalty in wartime Britain. In the present volume Lean is interested in the predispositions, social and psychological, that make alienation from the mother country possible. His subjects in this collective biography are about twenty Britishers who, from the time of Napoleon, have found loyalty to foreign leaders more appealing than support for John Bull's established governmental and ruling elites. The first three-quarters of this work are devoted to the study of twelve contemporaries of Napoleon whose loyalties to the emperor resisted the contrary pressures of public opinion and governmental policy.

The present work is not a sequel to Carl Cone's study of the political content of pro-Jacobin sentiment. Lean is not interested in political ideas and feelings as such. His primary aim is to uncover the abnormal reference points for the admiration of foreign rulers among a minority of Englishmen whom he would consider in other respects intelligent and rational. Each of the figures he examines was denied a normal adaptation to authority in later life by a difficult and unresolved relationship to a parent or parent substitute in childhood and adolescence. An unnatural dependence and submissiveness was imposed on the child who, because of admiration for paternal or maternal authority, had a "need to revolt" that he found "morally impossible" to fulfill. As a result, even as an adult the subject

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continued to work out his or her ambivalence toward authority. In the subjects Lean studies this effort at resolution was accomplished by a stance of quasi-permanent opposition, as in the classic cases of Charles James Fox and David Lloyd George, and by the adoption of surrogate father figures among foreign political leaders. The first four chapters examine the pro-Napoleonic stance of Charles James Fox, William Hazlitt, John Cam Hobhouse, Lord Byron, Lord and Lady Holland, Princess Caroline, Samuel Whitbread, Elizabeth Inchbald, and other lesser contemporaries. The concluding chapter analyzes the transfer of loyalty to figures like Mussolini, Hitler, and Stalin on the part of David Lloyd George, Sir Oswald Mosley, Kim Philby, Kingsley Martin, and a few other moderns.

Lean has examined the writings of his subjects for evidence of an ambivalent relationship to authority, a taste for rival courts, violence, theatricality, nomadism, and a host of other behaviors that he characterizes as the Napoleonic syndrome. The attack is far from systematic, and the reader will appreciate the unusually comprehensive subject-referenced index, which will remind him of the book's scattered allusions. The thesis itself is summarized (pp. 208–09) and expanded upon in the fourth chapter, "The Napoleonist Syndrome." This is the best chapter of the work in terms of cohesion, and it whets the reviewer's appetite for the publication of Sir Isaiah Berlin's Mellon Lectures (National Gallery, 1965), to which some debt is expressed in the preface.

Historians interested in a more social-scientific history will be drawn to Lean's search for syndromal constants in English political behavior but will be distressed by his methodology, his long lapses into a very unanalytic and old-fashioned narrative history, and the limits of his theorizing. Those in a more humanistic tradition will enjoy his relish for idiosyncratic detail and his ingenious use of the Holland House Papers. Both will bewail the lack of concern for citation of sources. The book should and will, nonetheless, find many readers in both camps. Alienation and the authoritarian personality, though not identified as such by the author, are central problems of our culture.

DANIEL P. RESNICK
Carnegie-Mellon University

J. F. BOSHER. *French Finances, 1770–1795: From Business to Bureaucracy.* (Cambridge Studies in Early Modern History.) New York: Cambridge University Press. 1970. Pp. xvi, 369. $18.50.

One need only compare the standard histories of the finances of the Old Regime and the French Revolution by Charles Gomel, Marcel Marion, and René Stourm with Professor J. F. Bosher's monograph to realize the originality and importance of its scholarly narrative. The standard histories deal for the most part with the agonizing efforts of the Old Regime monarchy and the revolutionary assemblies to keep fiscally afloat through taxes, emergency borrowings, management of expenses, partial repudiation of debt, and confiscation of property. The histories allude only in passing to Mr. Bosher's subject: the changing administrative machinery by which the revenues were collected and spent. In the closing decades of the Old Regime, Mr. Bosher observes the collecting and spending were in the hands of several hundred profit-seeking accountants who were independent of administrative control because they owned their offices. They mingled their private funds with government funds, used government funds in their private business ventures, and often themselves did not know on any given day what their fiscal situation was. No wonder the Old Regime monarchy could not bring order to its finances, prepare an annual budget, or substantially improve its financial situation. The ministers of finance in the 1770s, Terray, Turgot, and Necker, tried to rationalize the system by reducing the number of accountants, by appointing them from the professional class of superior clerks, and by insisting on regular fiscal reports, but the ministers of the 1780s, Joly de Fleury and Calonne, undid their work and restored the old anarchy. Then in 1788 Calonne's successor, Loménie de Brienne, in a reform relatively unnoticed by contemporaries and historians, founded a central Treasury of professional civil servants. During the years of revolutionary turmoil the Treasury was awarded by legislation all the fiscal operations of the central government. By 1795 the anarchy of private capitalistic accountants had been replaced by a professional bureaucracy of salaried administrators, hierarchically organized along definite lines of communication and command, receiving all public

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revenues and paying all government expenditures, proceeding by standard accounting practice, preparing reports open to public inspection, and capable of mobilizing the financial resources of the nation. The new Treasury was an instrument that Napoleon was glad to use and the Restoration governments to inherit and improve. Mr. Bosher has recovered this history, hitherto largely unknown, by patiently and skillfully dovetailing scattered evidence from archives, pamphlets, memoirs, newspapers, and speeches. His style of presentation, at once objective and warm, is perfectly adapted to the subject. The book is *solide* and offers conclusions that should endure.

HAROLD T. PARKER
Duke University

MAURICE AGULHON. *Une ville ouvrière au temps du socialisme utopique: Toulon de 1815 à 1851.* (École Pratique des Hautes Études—Sorbonne. VI[e] Section: Sciences économiques et sociales. Centre de Recherches Historiques. Civilisations et sociétés, Number 18.) Paris: Mouton. 1970. Pp. 368. 48 fr.

This fascinating book promises to become a classic in working-class historiography. It has been drawn from Agulhon's *thèse de doctorat,* a general sociocultural history of the Var department from 1800 to 1851. The other parts have been published under the titles *La Société et la vie sociale en Provence intérieure au lendemain de la Révolution* (1971) and *La République au village* (1970). This mode of publishing the normally massive French theses is most welcome. But there is nothing artificial about Agulhon's extraction of Toulon and its working-class population from the broader social history of the Var. Because of the determinative influence of the naval arsenal and port on its economic and social structures, Toulon was essentially a national rather than provincial city and, unlike its department, modernized rapidly during the first half of the nineteenth century. The city's resident population increased by at least sixty per cent (to nearly 50,000) from 1831 to 1846, placing it among the fastest growing cities in France. Simultaneously, the transformation of techniques of production and supply turned it into one of the few pre-1848 centers of the French industrial revolution. Private entrepreneurs supplying materiel and equipment, cabaret and *pension* owners, and the construction industry prospered as never before. And inevitably the wage-earning working class mushroomed. Toulon was indeed a *ville ouvrière* by 1848.

The author's fundamental purpose is to show how this population, socially deferential and even actively royalist in 1815, created by 1850 one of the principal democratic strongholds in the *Midi rouge*. What caused this republican and socialist *prise de conscience?* Agulhon's answers, or better, his mode of answering, are what give this work the allure of a classic even from the moment of its publication. Social historians deal with the comportment of masses of people over time. Because of the breadth of their task and the nature of their usual documentation, they often allow the correlations they discover among their data series to stand as explanations of social behavior and even, by further inference, of collective mentalities. But if one is interested in comportment and mentalities he must find out precisely how people relate, influence one another, and act together and what they actually believe as manifested by their recorded thoughts. It is only in the latter areas of research that correlations can become explanations, and few social historians have gathered such a wealth of this kind of information over so extensive an area or period. In both this work and the *République au village* one finds, besides the usual social and economic indicators, the detailed penetration of the movement, the rhythm, the personality of a society, and the human interaction within it. Hence folklore, popular theater, songs, poetry, and questions of education and language find their way into both books. More fundamental are modes of popular organization and their evolution. Crucial here are those primordial forms of sociability, the suburban *guignettes,* where tired Toulon workers gathered for drink, song, and discussion, and the similar *chambrées* in the rest of the department. For from these coteries sprang mutual benefit societies and other forms of working-class organization, and ultimately strikes and political clubs.

What conclusions about the emergence of a popular democratic consciousness in Toulon are drawn from such materials? A burgeoning population unrelieved by urban planning and

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technological advance promoting redundancy and bringing skilled, non-Provencal minds into the Toulon working class were radicalizing influences. The power structure, divided between a transient military and administrative elite and the native bourgeoisie, was less conducive to conservative control than elsewhere in Provence. Division itself created the potential of greater working-class power, but more important, the local bourgeoisie took an "oppositionist" orientation. This leads to Agulhon's most interesting and original conclusion. This *prise de conscience* was not achieved autonomously by the working class but developed in large measure under the tutelage of liberal bourgeois. The social humanitarianism and imaginative economics of Saint-Simonism appealed to "enlightened" (often Masonic) businessmen and professionals and was mainly responsible for this vertical ideological linkage. Agulhon found similar patterns in the *bourgs* and villages of the Var. Moreover, working-class and peasant tastes and social groupings often took the form of "social imitation." In this way Vendéen patterns of influence by conservative notables and priests were broken. The passage from Right to Left, "from the structure of patronage, which is conservative, to the egalitarian structure, which is democratic," was effected "by an intermediary step, a democratic patronage" (*La République au village*, 481; see also *Une ville ouvrière*, 332). The author also does not miss the role of the traditional *artisanat*, especially its journeymen, in the dissemination of radicalism. In this group, both in town and village, emerged the first cadres of political and syndical activism. From the *ouvriers civils* in Toulon spread the vision of a new society to the thousands of more conservative proletarians in the dockyards. The crucial turning point in this entire process came with the great strike at the arsenal in 1845. The "Red Toulon" of 1848 was thus prepared.

The social-democratic apprenticeship of the Toulon workingman evolved under the most varied of influences. The panorama presented by Agulhon is reminiscent of E. P. Thompson's *Making of the English Working Class* (1963). Both works are cultural history in an almost anthropological sense. But Agulhon leaves one more intellectually satisfied, and the reason is simple. In the local context his answers, founded upon a mass of local documents, are more convincing. This underlines a fact already recognized by most of our French colleagues. For a long while yet the best social history will be local or regional history, and in this genre Maurice Agulhon has few equals.

CHRISTOPHER H. JOHNSON
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A. W. RAITT. *Prosper Mérimée*. New York: Charles Scribner's Sons. 1970. Pp. 453. $15.00.

Raitt's life of Mérimée is an excellent refutation of that sturdiest of stereotypes—the alienated Romantic writer of the nineteenth century. Mérimée was successful and satisfied, rather than starving and disaffected; he served Louis-Philippe as inspector of historical monuments and Louis-Napoleon as senator. Even in his choice of a mistress—the wife of Delessert, the prefect of Paris—Mérimée revealed himself as the well-connected, accommodating artist.

Historians will be particularly interested in Raitt's three chapters on Mérimée's historical activities. The best of these is on the young Mérimée as historical novelist, where Raitt demonstrates how a writer of the Restoration was naturally drawn to historical subjects.

Another chapter treats Mérimée's little-known career as inspector of historical monuments. Here, Raitt is guilty of the pardonable shortcoming of crediting his subject with too much; when Mérimée was given his post in 1834, the major decisions and commitments had already been made. Raitt mistakenly credits Montalembert and Hugo with having inspired the movement to restore French monuments and misses the significance of the ideology of the July Monarchy that made Mérimée's appointment possible. Thus he correctly notes the extraordinary increase of expenditures for restoration, but incorrectly attributes them to Mérimée's pleas rather than to that passion of the men of the July Revolution for the restoration of historical France. Finally, Raitt deals with Mérimée as historian. Mérimée himself admitted he was an anecdotist and after reading Raitt's judicious selections from Mérimée's histories we can only concur with Saint-Beuve's judgment. "No one was ever more sparing of ideas in the proper sense of the word."

Yet Raitt insists that Mérimée was a historian

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and suggests that the neglect of him stems from his having wandered far afield by choosing as subjects Spain, Russia, and ancient Rome. The explanation for this deserved neglect lies more simply in Mérimée's mind. Can there be anything more wrongheaded than Mérimée's observation on nineteenth-century history than, "The Germans have committed the worst possible crime, they brought imagination into a subject where it has no place: history."

With bons mots like these to overcome, any effort to rehabilitate Mérimée as historian will fail. Fortunately, the author of *Carmen* and *Colomba* need not depend upon his histories for survival.

STANLEY MELLON
University of Illinois, Chicago Circle

LOUIS M. GREENBERG. *Sisters of Liberty: Marseille, Lyon, Paris and the Reaction to a Centralized State, 1868–1871.* (Harvard Historical Monographs, Number 62.) Cambridge, Mass.: Harvard University Press. 1971. Pp. 391. $12.00.

Professor Greenberg did not intend to write a history of the Commune of 1871. He gives us a fine study of the decentralist movement, of the bungling attempts by Paris to achieve association with the other great cities, and of decentralist traditions and activity in both Lyon and Marseille. His book affirms, correctly in my opinion, the primacy of decentralization in the great upheaval of 1871, so often interpreted as a socialist conspiracy. He shows the cultural, economic, and political particularism that became decentralism and that found its opportunity in the military defeat of the unitary state in 1870. Based upon extensive archival and newspaper research, the book reveals how sympathy for Parisian autonomy was offset in the provincial cities by the traditional antagonism to Parisian supremacy and arrogance.

Because Greenberg's book surveys the opinions of local notables and newspapers rather than public opinion as a whole, one cannot be sure how deep decentralist sentiments ran in 1870. He says that Paris, for instance, "had expected an elected assembly as its reward for the September 4 revolution." Thus, when insurrection came in March of 1871, the central committee of the National Guard naturally understood its obligation to call those anticipated elections. Yet the plebiscite of November 3, 1870, had given the Government of National Defense a vote of confidence (558,000 to 62,000) after its rejection of the radical demand for municipal elections on October 31. I do not mean to undermine Greenberg's thesis, which I share, but to illustrate that monographs can both illuminate larger issues and induce tunnel vision.

The book raises debatable issues because it seems to equate liberalism with decentralization by making all proponents of the unitary state illiberal, no matter whether on other issues they were liberal or something else. This distorts both our notion of nineteenth-century liberalism and the variety of reasons for the existence of the unitary state. As Professor Greenberg knows, the passion for decentralization was also associated with elitism, a desire not to be governed at all and especially not by a democratic regime. Ostensibly anarchistic, this elitism was actually authoritarian, which can account for why many decentralists became centralists once they touched power.

No doubt France was overcentralized by the nineteenth century, and Greenberg is right in his belief that a better balance between national and local authority would have been emancipatory. On the other hand, there is a mystique abroad today that would have it that local power is not simply more responsive to local needs but less corrupt than national power. The notion is particularly dear to those who know cities rather than villages. I wonder if an analogous assumption—that we necessarily experience greater liberty in political decentralization—is another myth born of our frustration over life in an increasingly complex society and economy.

ROGER L. WILLIAMS
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JOHN M. SHERWOOD. *Georges Mandel and the Third Republic.* Stanford: Stanford University Press. 1970. Pp. ix, 393. $12.95.

Mr. Sherwood has undertaken the difficult task of trying to understand the political career of the admittedly enigmatic Georges Mandel. The author is familiar with the major issues and minor intrigues of the Third Republic, has worked diligently with the available materials, and has interviewed several of Mandel's surviving contemporaries and associates. If the end

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product is not as searching, as profound, or as critical a biography as one might have hoped, the reason may be the elusive quality of the subject and the dearth of any personal papers to clarify some of the darker recesses of Mandel's mind.

Both Mandel's political career and his personal qualities combined to create a deep-seated antagonism toward him in all quarters. As early as 1917 he had earned the undying animosity of the Left when as Clemenceau's personal assistant he ruthlessly repressed all stirrings of defeatism, pacifism, and dissent. At that time, and in the 1920s, he was known as a die-hard patriot of authoritarian temperament, conservative social and economic views, and overweening political ambitions. Extreme right-wing nationalists and quasi fascists at first embraced him, a dubious honor and one enjoyed by few other French Jews, but they abandoned him when his intransigent views toward Nazi Germany in the 1930s became clear. Personally, he was cold, distant, arrogant, and contumelious. Secretive and more fond of intrigue and maneuver than even the average parliamentarian, he was caught more than once in outright duplicity. Even those who admired his single-minded devotion to France were repelled. He reminded various contemporaries of "a barracuda always ready to pounce," of an "adder," "a serpent," "a cobra." With all of this carefully established by the author, it is somewhat misleading, therefore, to attribute his failure to achieve a position of leadership to anti-Semitism or to say that his Jewish origins "helped prevent him from acting more decisively during the last years of the Third Republic." And to say that "the role of savior that had seemed destined for Mandel passed to the relatively unknown General de Gaulle" is completely out of focus. Mandel's courage and patriotism in 1940, and his eventual martyrdom at the hands of the French *milice*, cannot be denied, but they have perhaps obscured the author's perspective. One is also disturbed at various points in the narrative by a readiness to repeat verbatim anecdotes or statements derived from doubtful or suspect sources; some of these statements (for example, p. 25) inspire complete incredulity and should have been weeded out, certainly not quoted directly. Despite these and various other weaknesses, the book is a conscientious, informative, and comprehensive study that students of the Third Republic will read with interest.

JOEL COLTON
Duke University

ALAN S. MILWARD. *The New Order and the French Economy*. New York: Oxford University Press. 1970. Pp. viii, 320. $12.00.

This book is now the standard account of how the Germans exploited the French economy from 1940 to 1944. The author has used the intimate knowledge of the German documents that he gained in writing his excellent *German Economy At War* (1965) to examine contemporaneous events in France. He begins by asking if military conquest can bring an economic pay-off to the victor. Against the "liberal" view that it cannot, Milward finds that the Nazis did in fact net a large economic return from France. About half the book describes how the French economy as a whole fitted into the changing wartime German strategy and the methods by which the Germans used it. When at the beginning of 1942 Hitler had to abandon the Blitzkrieg strategy for a total war effort, the Germans began to press harder on France. Milward estimates that by 1943 Germany was using for her own purposes over forty per cent of France's resources.

The author devotes special chapters to German exploitation of French coal, iron ore, bauxite and aluminum, wolfram, and agriculture, but unfortunately he does not probe deeply into various sectors of manufacturing, such as aircraft, where there is a story yet to be told. Among the interesting points Milward brings out is the number of times that Vichy officials tried to hold back French businessmen eager to deal with the occupant; the controversies within the German leadership, personified by Sauckel and Speer, over the German strategy of exploitation; the importance of transport problems in moving French goods to Germany late in the war; the contrast between rising labor productivity in Germany and declining productivity in France; France's replacement of Russia as a breadbasket for Germany; and finally, how the orientation of German wartime trade toward France and the Low Countries (rather than Eastern and Southeast-

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ern Europe) has continued under quite different auspices in the postwar years.

Milward has drawn his evidence largely from unpublished German documents, many of them available at the Imperial War Museum in London, and from published French records on war damage and on negotiations at the Armistice Commission meetings. His closeness to these bureaucratic documents may have affected his literary style, which does not sparkle.

JAMES M. LAUX
University of Cincinnati

HENRI MENDRAS. *The Vanishing Peasant: Innovation and Change in French Agriculture.* Translated by JEAN LERNER. (M.I.T. Studies in Comparative Politics.) Cambridge, Mass.: MIT Press. 1970. Pp. viii, 289. $8.95.

This elongated essay does not come from the pen of a historian. Henri Mendras is France's leading rural sociologist; for twenty years he has worked in that remarkable laboratory of social change that is the French countryside. His book, he warns, is not a synthesis of a vast and complex subject, but merely a pause in research to permit some tentative conclusions before striking out anew. Tentative or not, it is rich in insights, and its usefulness will not be confined to sociologists or to students of contemporary France alone. Indeed, Mendras argues that "one or two billion peasants at the threshold of industrial civilization . . . is the major world problem facing the social sciences" during the rest of this century, and he believes that the example of Western peasants, who have already made the transition to the industrial age, can offer guidance to peasants elsewhere.

It is Mendras's view that when a peasantry is transmuted into a stratum of modern capitalist farmers, whose new social and economic structures are patterned on those of industry, an entire civilization disappears. This change he views with concern, though he fears that government officials and farm leaders "brought up on liberal economics and Marxism" may not be able to conceive of any other objective. It is the sociologist's task, he says, to point out other possible alternatives, better suited to the special characteristics of agriculture. Although he proposes no precise blueprint, he clearly leans toward a kind of third way different both from the old subsistence family farm and from the large, industrialized food-producing enterprise. As the key institution he hypothesizes the small "workshop" of up to ten farmers, specializing in one product, either artisanal or cooperative in structure, and meshed with hundreds of other workshops in either horizontal or vertical fashion. Along with this structural change, Mendras foresees a new set of rural social relationships: a reinvigoration of towns and small cities, the "birth of a new rural bourgeoisie" that will bring together town dwellers and surrounding farmers in a closer symbiotic relationship. The traditional gulf between rural and big-city life will be narrowed; they will differ but will no longer confront each other in the form of two basically contradictory civilizations.

Mendras holds out no utopian certainties; he is only sure that "the last vestiges of the peasantry in France" will not survive the present younger generation, and he hopes that "the farmers' capacity for social invention," their "courage, imagination, spirit of enterprise," will find sustenance and sympathy from the nation's policy makers. Interspersed with these broad speculations, Mendras provides us with some fascinating glimpses into the processes and mechanisms of rural change in the France of our time.

GORDON WRIGHT
Stanford University

SIR CHARLES PETRIE. *King Charles III of Spain: An Enlightened Despot.* New York: John Day Company. 1971. Pp. xxi, 241. $7.95.

Writers in English have long neglected one of the most significant of the so-called "enlightened despots," Charles III, generally thought to be the greatest of the Bourbon monarchs of Spain. Since Charles has not had an English biographer for half a century or more, there is an opportunity to fill a substantial gap.

Charles's biographer would seem to have three closely associated problems. How did this obsessively routinarian, conventionally pious, traditionally educated monarch come to be the sponsor of far-reaching reforms? What sort of relation did Charles have with his ministers in conceiving and carrying through such changes? Finally, just how did these reforms affect the lives of Spaniards then and in the future?

Sir Charles Petrie allots about forty per cent

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of his pages to an introduction that embraces the advent of Philip V and his political and domestic difficulties. He then introduces Charles to kingship in Sicily before circling back to Ferdinand VI and the Spanish imperial system. The balance of the book is then devoted to Charles's rule proper, with very heavy emphasis upon foreign affairs and war. Only parts of two chapters are reserved for purely internal developments. Petrie deals successively with Spain's attempts to get her house in order after 1763, the disastrous Algiers expedition, and, finally, the War of American Independence.

The author's approach to his material is essentially narrative. He is good at telling a story, and his prose is lucid and easy. He enlivens his tale with amusing anecdotes, which occasionally seem not particularly apropos, and he has a forgivable weakness for relating the adventures of wandering Irishmen in Spanish service. However, he sticks mainly to court and cabinet.

This strong focus on war and diplomacy is, to my mind, one of the chief defects of his book. He fails to say much about economic matters. He conveys not even an inkling of the arduous efforts to industrialize and to improve agriculture, trade, and transportation that so much occupied Charles and his ministers. Indeed, domestic affairs in general, except for the expulsion of the Jesuits and the misadventures of Pablo Olavide, get rather short shift. All that ferment of useful projects, from canals to public spectacles, so typical of Spanish reform, is passed over. More important no one reading Petrie's book would come away with any idea that Charles III's reign was a time of intellectual ferment. The impact of the Enlightenment and the relationship of new ideas to Charles's reforms are totally neglected.

To judge from the limited documentation, moreover, the author is unaware of the work of important authors such as Sarrailh, Corona, Domínguez Ortiz, Rodríguez Casado, and Herr in Spanish history or of Lynch or Caughey in the colonial field.

Because of these lacunae Petrie is unable to give a complete picture of the political impact of Charles III, and this, in turn, keeps him from any significant inquiry into the motivation and direction of the Caroline reforms. Petrie's book, then, is a pleasant summary of court and diplomatic matters but without much originality and with no analysis of the formative social or economic forces of Charles's reign.

GEORGE M. ADDY
Brigham Young University

RICHARD A. H. ROBINSON. *The Origins of Franco's Spain: The Right, the Republic and Revolution, 1931–1936.* (Library of Politics and Society.) [Pittsburgh:] University of Pittsburgh Press. 1970. Pp. 475. $9.95.

The Spanish Civil War of 1936–39 has attracted many historians; studies of its origins have not proved as compelling, partly because of the complexity of events and the lack of solid documentation. Professor Robinson of the University of Birmingham has approached and solved both of these problems. His aim was to write a history of the Right—principally the CEDA and the monarchists—from 1931 to 1936. In order to put their development into context he had to deal with the Left and Center as well. The result goes beyond his title; his book is actually a complete political history of the Second Republic and is certainly the most useful and satisfying study of that period yet published.

Robinson's principal source is the rightist press—particularly the Catholic *El Debate* and the monarchist *ABC*. His skillful use of these readily available sources demonstrates that despite other manifestations of backwardness the Spanish press was one of Europe's most sophisticated, and there is much valuable material to be gleaned from it. He was also able to use the recently published memoirs of the *jefe* of the CEDA, Gil Robles, a most valuable and indispensable volume on the fortunes of the CEDA.

The development of the Right from its monarchist to its "accidentalist" extremes is shown in meticulous detail. Robinson's chief conclusion is that "given that the future of the Republic depended on the Socialist movement and the Catholic party, it is important to recognize that it was the former and not the latter which abandoned democratic methods and appealed to violence." Gil Robles never gave up his insistence on legality, but the Left refused to believe him; yet it was the Left that rose against the Republic in 1934 and contributed to the atmosphere of violence in the spring of 1936. His other conclusion is that "it is clear

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that the Left Republicans themselves dealt the democratic Republic a serious blow by equating the form of government with their own ideological predilections."

What emerges from this study is the concern all the Spanish parties had with form rather than substance. The extreme Right refused to accept a republic, no matter how conservative, and the Socialists refused to accept a bourgeois republic, no matter how reformist. "Accidentialism" and "possibilism" were terms not restricted to the political vocabulary of the Right alone. The Socialists' refusal to believe Gil Robles and the ideological anticlericalism of the Left Republicans prevented the kind of partnership that would have begun a solution to Spain's problems and prevented war. Manuel Azaña, himself the leader of the Left Republicans, posed the problem of modern Spain in a speech in 1930: "Just as there are some persons who suffer from hereditary diseases, so Spain, as a nation, suffers from 'hereditary history.' "

JOSÉ M. SÁNCHEZ
Saint Louis University

JOAQUIM ANTERO ROMERO MAGALHÃES. *Para o estudo de Algarve económico durante o século XVI.* (A marcha da humanidade, Section: Para a história de Portugal e Brasil.) Lisbon: Edições Cosmos. 1970. Pp. 288.

The southernmost portion of Portugal, a narrow landmass lying along the shore of the Mediterranean and separated from the rest of Portugal by mountains, faces Africa and is appropriately called by an Arabic name, Algarve, from *al-gharb,* "the west." In the opinion of the author of this volume, the proximity of the Algarve to North Africa and its geographic isolation from other parts of the country have determined its history. Romero Magalhães has written an excellent study of the economy of this region in the sixteenth century. He has done considerable pioneer archival research, especially in local municipal archives, and has made skillful use of the few narrative sources and sparse printed documentation. There are, according to the author, really two Algarves: a thinly populated and underdeveloped sierra region and an extensively cultivated foothill and agricultural plain. In the sixteenth century as in the Middle Ages the agricultural crops grown on the scarplands and plateaus of the *barrocal* and on the alluvial lands along the Mediterranean shore, in addition to the products of the coastal fisheries, were highly solicited in foreign markets and bound the region to an export economy. Magalhães modifies the usual interpretation of the Algarve's economic decline at the end of the sixteenth century in two ways. In the first place, he finds that certain parts of the economy, notably the tuna fisheries, were at the height of their development precisely at the end of the century. Second, it was not Spanish domination from 1580 on that weighed heavily on this region, as earlier writers have held, but rather such Europe-wide phenomena as demographic growth and the Price Revolution. A combination of food shortages, high prices, and low wages brought increasing misery to the working classes. Under such adverse economic conditions land became the best possible investment; the result was its further monopolization by the class that originally possessed it—the *fidalgos,* or lower nobility. All this led eventually to a greater polarization between the wealthy and noble, who controlled land and office, on top and a mass of impoverished peasants and workers on the bottom.

My principal reservations about this work relate to the author's total reliance on Fernand Braudel and his hesitancy to strike out on his own. This dependence is probably due to the fact that the book was originally written as a thesis, but in too many instances the reader is left dissatisfied and expecting more from the documentation. Despite these limitations, however, the volume is a fine introduction to a relatively unstudied area; regional studies like this one will surely do much to bring about a better understanding of Portuguese history in the early modern era.

RUTH PIKE
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C. O. BØGGILD-ANDERSEN. *Hannibal Sehested: En dansk Statsmand* [Hannibal Sehested: A Danish Statesman]. Volume 1, *Manden og tiden, 1609–1666; Indsats i dansk-norsk politik, 1636–1660* [The Man and His Times, 1609–1666; Contribution to Danish-Norwegian Policy, 1636–1660]; Volume 2, *Danmark-Norges rigsskatmester, 1660–1666; Indsats i dansk-norsk ydrepolitik, 1660–1666* [Treasurer of the Realm of Den-

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mark-Norway, 1660–1666; Contribution to Danish-Norwegian Foreign Policy, 1660–1666], edited for Jysk Selskab for Historie by OLE DEGN. Copenhagen: H. Hirschsprungs Forlag; Aarhus: Universitetsforlaget. 1946; 1970. Pp. 629; xx, 604.

Twenty-four years and the death of the author separate volume 1 from volume 2 of this monumental biography of a Danish statesman who flourished in the middle of the seventeenth century. Carl Olof Bøggild-Andersen was librarian at the Royal Library in Copenhagen from 1933 until 1939, during which time he completed his doctoral dissertation on the Danish Revolution of 1660. He became professor at the University of Aarhus in 1939 and continued his research on the critical period of the history of Denmark that saw the transformation of the monarchy from elective to hereditary, radical changes in power of the nobility, the beginnings of a new system of unparalleled royal absolutism, the loss of the rich eastern provinces to Sweden, and the reduction in the relative importance of the Danish position in Northern Europe. The long interval is in part to be accounted for by the teaching activities of the author, and in part by the fact that the first volume was given a very perceptive and incisive, though not in any way destructive, critique by Ingvar Andersson in *Historisk Tidsskrift* (ser. 11, vol. 2 [1947–49]: 405–34). Andersson, for many years the national archivist of Sweden, criticized the first volume for not truly demonstrating, on the basis of the source materials, that Sehested could really be counted the most important single figure in Danish history during this period. To do this, Andersson argued, was to force the sources. Bøggild-Andersen's response to the criticism was to recognize that before he could proceed with volume 2, which in the preface to volume 1 he argued was ready to be published, he had to undertake further research in a postwar Europe that had now reopened its archives. Although he did do this, the manuscript was insubstantially changed, and the years passed without its appearance. When the Jutland Historical Society determined to publish the second volume, it was because Bøggild-Andersen, before his death, declared his intention of doing so, and quite properly the editor, Ole Degn, has added to it a series of lectures on Hannibal Sehested and Danish foreign policy, 1660–66, which Bøggild-Andersen delivered at the University of Gothenburg in 1949. These constitute pages 383–486 and represent his defense of his earlier thesis.

Sehested was the ninth child of a nobleman from Jutland and was born in 1609 in Ösel, then a Danish possession but later lost to Sweden in 1645. His abilities attracted the attention of Christian IV, who gave him one of his daughters in marriage. He thereby became one of the "brothers-in-law" so important in Danish politics in the 1640s. He was named councillor of the realm in 1640 and viceroy (*Statholder*) in Norway in 1642. When war broke out with Sweden in 1643, he was instrumental in leading Norwegian attacks on Sweden and, by means of his careful planning and great organizational ability, securing considerable success. After the death of Christian IV in 1648, he was accused by personal enemies and unjustly, in the opinion of Bøggild-Andersen, of fraud and was dismissed from the Council of the Realm and his position in Norway by Frederick III in 1651. He was forced to relinquish his Norwegian properties and went into a wandering exile until 1657, staying in Holstein and in Hamburg but also visiting England, the Low Countries, and Spain. He returned to find his country once again involved in war with Sweden, a war that ended disastrously for Denmark with the loss in 1658 not only of all the provinces east of the Sound but also of Bornholm and Trondeheim.

During the second war with Sweden, which followed almost immediately, Sehested was a prisoner of the Swedish king and subsequently played a role in the negotiations that ended the unsuccessful Swedish attempt to conquer Denmark and led to the peace of Copenhagen in 1660. The subsequent Danish constitutional changes brought Sehested to the fore as a reformer of Danish state finances in his office of treasurer of the realm. Swedish models for the new collegiate system of administration proved effective also in Denmark, and Sehested contributed to making the new hereditary and absolute monarchy fiscally sound. Sehested was also fundamentally interested in foreign policy and engaged in diplomatic missions to England, the Netherlands, and France. In France where he died in 1666, he worked closely with Abbé Jean-Baptiste Paulmyer who, by compos-

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ing a "treatment" consisting of various statements he had heard Sehested make, is responsible for much of Sehested's later reputation. Bøggild-Andersen values the testament highly and believes the sentiments expressed in it to be consistent with earlier statements of Sehested. Sehested argued for an understanding between Denmark and Sweden that would preclude any attempt to recover the provinces lost and could, by granting Sweden superiority on land and Denmark at sea, provide the basis for peace and independence in the North. Together these two volumes are a solid foundation for further investigation of the foreign and domestic policies of Denmark at a time when that country, along with Sweden, could still be reckoned among the important powers of Europe. A good table of contents and excellent indexes in volume 2 make the somewhat heavy (together almost seven pounds) books quite easy to use.

ERNST EKMAN
University of California, Riverside

W. GLYN JONES. *Denmark*. (Nations of the Modern World.) New York: Praeger Publishers. 1970. Pp. 256. $8.00.

One might reasonably expect from the literary historian turned general historian a volume devoted to those basic patterns of social and economic life essential to an understanding of cultural developments—with just enough of the political story thrown in to satisfy the curiosity of his readers. Instead, W. Glyn Jones, an able British student of Scandinavian literature, has chosen to write what is essentially a record of political events from the middle ages and of constitutional and party strife in the past century and a half—with only enough analysis of economic and social forces to make a succession of ministerial crises and elections intelligible. In addition he has served up brief summaries of cultural activity, chiefly literary, at the ends of some of the nine chapters, and has added useful appendixes dealing with recent social legislation, the Faroe Islands, Greenland, the Danish West Indies, and the rulers of Denmark.

Few political historians, in writing a book for the elusive "general reader," would dare to offer such fare, certainly not in discussing the land of Hans Christian Andersen, Bishop Grundtvig, Søren Kierkegaard, and Kaj Munch. But the author of this volume has at least dodged a common tendency: either to dwell overlong on the undeniable charms of Copenhagen and the Danish countryside or to imply that the Danes walked suddenly into their welfare state and advanced democracy after deciding upon Marquis Childs's "middle way." And he has done this very well, making excellent use of the scholarly writings of Aage Friis, John Danstrup, Hal Koch, R. Andersen, Ib Koch-Olsen, and others.

The book begins with a survey of Danish history from about 850 A.D. to the conclusion of the Napoleonic wars. If this chapter presents something of an obstacle course to the uninitiated, it also offers a basis for understanding the Treaty of Kiel. Glyn Jones then discusses the growth of liberalism and nationalism after 1814 and, in the process, necessarily struggles with the Schleswig-Holstein question.

With the crucial year 1864 the book takes on genuine interest, dealing as it does with the transformation of a feudalized society and an absolutist-elitist government into a nation of yeoman farmers with a parliamentary system responsive to pressures for greater political and social democracy. It deals ably with the World War I period but less so with the World War II years, gives proper emphasis to the country's economic vulnerability because of its dependence on agricultural exports, discusses adequately Denmark's enlightened and humane social legislation, and describes clearly the reasons for her conduct in international affairs. It also suggests the problems related to Denmark's likely entry into the Common Market.

If the general plan of the volume, with its emphasis on party struggles and ministerial difficulties, is a bit demanding of the general reader, it does indicate the many stresses and strains that have attended Denmark's development in the past century. The same objective might have been more effectively achieved by a modified topical approach, but the fact remains that Glyn Jones has produced a brief history in English that may be used with confidence by scholar and general reader alike.

KENNETH O. BJORK
St. Olaf College

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SVERKER OREDSSON. *Järnvägarna och det allmänna: Svensk järnvägspolitik fram till 1890* [Railways and Government: Swedish Railway Policy to 1890]. (Bibliotheca Historica Lundensis, Number 24.) Lund: Gleerups. 1969. Pp. 336.

The first Swedish railway enterprise, that of Count von Rosen in the early 1850s, was a financial disaster. From that time forth for the remainder of the century railway development was accompanied by a continuing debate over the relative merits of private and public enterprise. While providing much ammunition for argument, the experience of other countries was of very little help to the Swedes in deciding their own policy. Basically, the argument for state enterprise was that it provided greater financial solidity and permitted a more rational planning of the network. Against this it was held that state enterprise was financially wasteful and would bring a heavy burden of debt upon the state. Subsidiary arguments concerned relative profitability, direct and indirect, to the economy. In the period covered by Oredsson's book no definitive solution was ever found. An initial reaction to von Rosen's failure brought a period of active building by the state, which lasted until about 1870. This gave way to renewed emphasis on private building, until the depression of the late 1870s and 1880s brought more bankruptcies and a new period of state activity, concerned this time mainly with north-south trunk routes. By 1890 Sweden had a curious mixture of state and private railway lines.

Oredsson gives a brief account of the twists and reversals in government policy and goes on to devote the major part of his book to a systematic examination of the arguments used at each stage, the make-up of the various interest groups involved, and their ideologies. Emphasis throughout is on ideas and arguments; material interests come in only as background information, and the decision-making process is scarcely touched upon at all. Using the analytical approach of R. A. Levine, Oredsson dissects arguments as to definitions, evaluations, descriptions of reality, and any recommendations the arguments might lead to. Consequently, although one learns much about the debate, what bearing (if any) it all had on the decisions that were made never emerges. This is a useful study, but it contains more formal organization than the material requires. At one point Oredsson tries to present his analysis in tabular form, but in explaining how the table is to be read he in fact gives the analysis it contains in plain, simple words: Swedish *passion géometrique* carried to absurdity.

There is a short summary in English.

G. M. SCHWARZ
Memorial University of Newfoundland

ROLF TORSTENDAHL. *Mellan nykonservatism och liberalism: Idébrytningar inom högern och bondepartierna 1918–1934* [Between New Conservatism and Liberalism: Ideological Split within the Conservative and the Agrarian Parties, 1918–1934]. (Scandinavian University Books. Studia Historica Upsaliensia, Number 29.) [Stockholm: Läromedelsförlagen.] 1969. Pp. 230. 38 S. kr.

Conservatives (Höger) and Agrarians (Bondepartiet), stuck to the right of Liberals and Social Democrats, faced a rapidly changing Swedish society and economy with no political ammunition. If either party used special interests as a reason for existence, accepted too much liberalism, defended democracy too vigorously, pledged themselves to reforms or posited set principles, they automatically admitted the greater philosophical and pragmatic weight of their opponents. Therefore, political ideology and practical considerations played an enormously important role in party discussions and led to ideological contradictions in view of Sweden's background of crises and the changing politics of Europe. If the Conservatives leaned too heavily upon nationalism or the corporate state, what then divided them from fascism or nazism? If they solidly supported Swedish economic interests, what then happened with farmer-banker antagonisms or the middle-class opposition to monopoly? These and other considerations caused breaches within each party and between the Conservatives and Agrarians, leading to the breakaway of the Young Conservatives and dividing the two parties to this day. Yet, in the end, both organizations deserted their ideologies and positions after 1934, the Agrarians to support the Social Democrats and the Conservatives to accept liberalism and even state welfare planning. It is this contradictory mixture of ideas that Rolf

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Torstendahl examines as a part of politico-intellectual movements within Sweden during the sixteen years after World War I.

Because of the welter of ideas contradictions creep into the narrative—even the English summary—where statements of a few leaders are made into general themes or where the antidemocratic words of some persons imply opposition to parliamentary government. The racist doctrines of Nazi Germany appear as Conservative concepts, despite the party's insistent denial of such charges, and are confused with xenophobia and anti-immigration propaganda. Actually, the last chapter of summary conclusions might have been excellent as an introduction, for its reasoned balance makes the work intelligible.

Other aspects also weaken the effectiveness of the study as an inquiry of a transitional period for Sweden and two of its parties. The proportionate imbalance of attention between Conservatives and Agrarians leaves gaps, as do contradictions in definitions of individualism and individual freedom, especially when the term collectivist is applied to those who believed in an organic state. The bibliography covers all possible sources for both parties, but the index is a sorry excuse with its use of only proper names and its omission of subjects.

RAYMOND E. LINDGREN
California State College, Long Beach

YRJÖ KAUKIAINEN. *Suomen talonpoikaispurjehdus 1800–luvun alkupuoliskolla (1810–1853)* [The Maritime Trade of Peasants and Other Coastal Dwellers in Finland (1810–1853)]. (Historiallisia Tutkimuksia, Number 79.) Helsinki: Suomen Historiallinen Seura. 1970. Pp. 262, 6 tables.

Kaukiainen's work is a quite narrowly circumscribed study of the mercantile history of Finland. The Finnish title gives a more accurate description of its scope than the English translation of it, since the author deliberately excludes the urban practitioners of maritime trade from his investigation. After a brief historical survey of maritime trade conducted by the coastal peasantry of Finland, in which he also pays some attention to similiar phenomenon in Sweden and Norway, he proceeds to discuss various aspects of his topic. He dwells on the consequences of the separation of Finland from Sweden on the trade, ownership, and manning of vessels, on engagement in shipbuilding, and on the significance of the trade to the local and national economy. Some of his answers to his own questions are to be expected. Political separation of Finland from Sweden eventually reduced this type of trade between them and while the coastal peasants in the west concentrated on commercial connections to Stockholm, those of the south coast sought them with Estonia: the Karelians in the east sailed to St. Petersburg.

Kaukiainen's topic suggests several paths of exploration that he has not followed or only mentions briefly. The relationship between the rural maritime trade and the development of commerce and shipping in the coastal towns and cities of Finland is barely touched upon. The cultural and social significance of the voyages and the trade activity is only referred to in a few paragraphs. It might have been interesting, for example, to learn something about the effect of peasant seafaring on social mobility. To what extent did part-time coastal peasant entrepreneurs or their children join the ranks of town merchants or how else did their accumulated wealth and experiences affect their standing in society?

In summary this is in its approach a quite traditional type of study in economic history. While it fails to answer many questions that it raises, it does offer a good deal of new material.

PEKKA KALEVI HAMALAINEN
University of Wisconsin, Madison

J. E. O. SCREEN. *Mannerheim: The Years of Preparation*. London: C. Hurst and Company. 1970. Pp. ix, 158. £2.75.

Biography of recent historical figures is always difficult, but when the subject is Baron Carl Gustaf Mannerheim the problems are manifold. Cloaked in myths, dead only twenty years, his private papers closed to all but one scholar, he is a formidable challenge. Moreover, he strode across the Finnish stage as "white general," regent, field marshal, and president after having served the Russian tsars for thirty years and after having already reached the age of fifty. It is Mr. Screen's thesis that Mannerheim's Russian experience cannot be dismissed

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or forgotten as has been done by so many of his biographers.

Screen's study of the early Mannerheim years is not wholly new, but it is the first book-length treatment in English. Marvin Rintala has described Mannerheim's coming to Finland in 1917 as a refugee with eyes still fixed on Russia and with political views only less hostile to Finnish republicans, democrats, and parliamentarians than to Russian Bolsheviks. Contrarily, Stig Jägerskiöld in the first two volumes of his evolving biography portrays the ex-tsarist general as thoroughly Finnish in feelings and liberally constitutional in politics. Screen is midway between these two. Familiar with the available sources, he documents Mannerheim's deep, enduring attachment to tsarist Russia but does not find him alien to Finland. Though a prewar "compliant," Mannerheim opposed Russification; though anxious to commit Finland to intervention against Bolshevik Russia, he vigorously asserted Finnish national interests at Paris, in the Åland Islands, and in Karelia. Strong advocate of a monarchical Finland with a powerful executive and oligarchy, he nevertheless was a constitutionalist, albeit hardly a liberal one. Finally, though European aristocrat and professional soldier, his Finnish origins were unmistakably evident.

Beginning with a valuable bibliographical survey, the author, a University of London librarian, has written a careful and thoughtful appraisal of how "the years of preparation" in Russia helped to shape this highly eminent political and military personality of modern Finland.

KENT FORSTER
Pennsylvania State University

KEIJO K. KULHA. *Karjalaisen siirtoväen asuttamisesta käyty julkinen keskustelu vuosina 1944–1948* [Public Discussion of the Settlement of the Displaced Karelian Population, 1944–1948]. (Studia Historica Jyväskyläensia, Number 7.) Jyväskylä: [K. J. Gummerus Osakeyhtiön.] 1969. Pp. 317.

This doctoral dissertation from the University of Jyväskylä, Finland, analyzes the emergence and solution of a postwar political problem in Finland. The Soviet victory over Germany forced Finland to cede approximately ten per cent of her state territory, including a large part of the southeastern region known commonly as Karelia. The Finnish Parliament recognized the nation's obligation to recompensate for their lost properties the approximately 400,000 refugees from Karelia, most of whom were farmers and foresters. Elaborate legislative provisions stipulated the establishment of the proper administrative agencies, their functions, the ways of the acquisition, which was compulsory from the original proprietors' point of view, of some 250,000 hectares of land. Discharged war veterans were included in the land grant part of the program.

In the preparatory phase of the Karelian refugee settlement program, all interested social, political, and economic groups had the opportunity to express their views, with the exception of a right-wing party, which was abolished on the basis of the temporary peace treaty of 1944. The Communists, a newly admitted parliamentary group, made their first attempt at the application of democratic rules in their political game in Finland. Academic experts as well as professional opinion makers in the Finnish- and Swedish-language press and radio of Finland (many of whom identified with one or the other political organization, not excluding those of the prospective land recipients and sellers) contributed vigorously to the public debate, which has been eyed watchfully by the policy makers as well as by the public.

Mr. Kulha's presentation is well organized and convincing. His characterizations of the refugee groups and other land recipients as well as of the other participants in the debate are full of useful information. His surveys of the land acquisition procedures, the respective legislative measures, and other related issues are reliable. He might overemphasize the importance of the stand taken by the Swedish minority group, whose properties in the west of the country were affected by the planners, and by the press and party organization of the Social Democrats, who sided with the Swedes against the Agrarian party, which promoted the interests of the refugees.

Since the author takes issue with the repeated comments of the press in Sweden (which also aided the Swedish ethnic group in Finland), it might have been of interest to see a comparative survey of the Finnish language press in North America, which, to my knowl-

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edge, also took up positions according to the political motivations of the individual newspapers.

What accounts for a more serious gap in the scholarly texture of the work, the probable lack of an intimate knowledge of the respective publications by Finnish as well as by foreign authors in English and other Western European languages—cannot be left unmentioned. Consequently, the bibliography at the end of the volume appears to be incomplete from the American and Western European point of view. This weakness is corrected only in part by the inclusion of a rather lengthy summary in English, the language of which is clearly below the academic level of this worthy book.

ELEMER BAKO
Silver Spring, Maryland

KYÖSTI JULKU. *Die revolutionäre Bewegung im Rheinland am Ende des achtzehnten Jahrhunderts.* Volume 2, *Die Revolution im Rheinland während der Franzosenherrschaft (1792 bis 1801).* (Suomalaisen Tiedeakatemian Toimituksia, Series B, Number 148.) Helsinki: Suomalainen Tiedeakatemia; distrib. by Akateeminen Kirjakauppa, Helsinki. 1969. Pp. 277. 33 F. Mk.

This second and concluding volume of the author's study of the revolutionary movement in the Rhineland at the end of the eighteenth century covers the period of French domination from 1792 to 1801. Along with Jacques Droz's older study, *L'Allemagne et la Révolution française* (1949), Julku's work ranks as the best on the subject. In certain respects, as in his treatment of the press, Julku even surpasses Droz. As against Droz's contention, for example, that an analysis of the press indicates a waning of revolutionary enthusiasm in the Rhineland as early as 1791, Julku shows that the changes in the press's political attitudes were due more to the establishment of a stricter censorship, to the immediate social and intellectual impact of the Revolution, and to the rise of a stronger current of reaction in Germany than to any voluntary relapse on the part of Rhineland newspaper publishers or the public at large.

Throughout, the author focuses on the complicated interplay between conflicting political and cultural forces, both foreign and domestic, that produced the revolutionary movement in the Rhineland. Like the historically rich and diverse land of its birth, the Rhineland revolutionary movement assumed, in the course of its development, the qualities of a microcosm, a microcosm of the European revolutionary movement in general. "For the formation of the outlook of these Enlightenment revolutionaries, the political and cultural cross-current was of the greatest significance." Influenced by both the French and German versions of the Enlightenment and torn between the wave of radicalism emanating from Paris and a resurgent conservatism originating in Vienna, the Rhineland revolutionaries found themselves at the very center of a conflict involving forces traditional and modern, Eastern and Western, religious and secular, progressive and reactionary. Unwilling and unable to accept either set to the exclusion of the other, they sought always to reconcile the two. "No French influence was accepted automatically in the Rhineland; something of its own was always added. A synthesis of the French and German Enlightenments was the objective." What Julku provides is a detailed, balanced, well-documented account of that synthesis.

ROBERT ANCHOR
University of Southern California

ULRICH NETH. *Standesherren und liberale Bewegung: Der Kampf des württembergischen standesherrlichen Adels um seine Rechtsstellung in der zweiten Hälfte des 19. Jahrhunderts.* (Schriften zur südwestdeutschen Landeskunde, Number 9.) Stuttgart: Müller & Gräff. 1970. Pp. xx, 372. DM 15.

The nobility of nineteenth-century Germany has seldom attracted the attention of professional historians, and the literature that has examined the structure of this powerful elite is singularly devoid of scholarly works of distinction. There are, to be sure, familial almanacs by genealogical enthusiasts that are exemplars of literary narcosis but that are of limited use to historians. The mediatized nobility, however, has found an urbane and perceptive analysis in Heinz Gollwitzer's *Die Standesherren: Die politische und gesellschaftliche Stellung der Mediatisierten, 1815–1918* (2d. ed.; 1964). Neth's industrious but more modest study, based on archival and printed materials, subjects to microscopic scrutiny the relationship of the mediatized princes and counts resident in

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Württemberg and the government of that state after 1806. His analysis unfortunately bears inelegant marks of the dissertation seminar and provides more detail on the declining status of the Württemberg *Standesherren* than even a present-day princeling of Thurn und Taxis in dutiful pursuit of his Swabian ancestors might reasonably be expected to want.

No German sovereigns were more hostile to the mediatized nobles than the kings of Württemberg. As early as 1806 Frederick I attempted to abrogate many rights reserved to the mediatized by the Confederation of the Rhine. Article 14 of the *Bundesakte* of 1815 frustrated this ambition by guaranteeing these privileges, which were grudgingly recognized by the Crown in the Constitution of 1819. In the revolutionary turmoil of 1848–49 the peasant dues owed to all landlords were commuted for cash compensation. In addition, the mediatized were deprived of many of the ceremonial and administrative prerogatives that they formerly had enjoyed on their domains. Protests to the king and to the Diet of the Germanic Confederation followed. After long debate with the Württemberg government a definitive settlement was reached in 1865. This adjusted the indemnity for the loss of peasant dues, restored many of the domiciliary rights, but restricted the mediatized to a feeble role in politics. Most of the salvaged privileges were eroded by the Crown between 1865 and 1918 so that the *coup de grâce* prescribed by the Constitution of 1919 was only a symbolic ritual of republican equalitarianism.

A book on the mediatized, even one restricted to those resident in Württemberg, would be welcome, but Neth's is not the one we need. His account of the struggle between the state and the *Standesherren* does not tell us enough about either. We do not learn why Württemberg's handling of these nobles should have been more punitive than that of other states, nor are we ever given a systematic analysis of the economic position of the mediatized. This is especially unfortunate because, as the author points out, no prerogatives were more cherished by the mediatized than those that protected their positions as landlords. We need to know how much land the *Standesherren* possessed, the extent to which it was entailed, how it was managed, the income it produced, and how variations in these factors from noble house to noble house affected the patterns of reaction to the state's assaults. Until these matters are made clear we are not ready to proceed to an understanding of why the mediatized were so determined to preserve their special rights and why they were therefore bound to collide with a state equally insistent on the limitation and eventual eradication of such privileges.

LAMAR CECIL
University of North Carolina, Chapel Hill

HEINZ HERMANNS. *Die Handelskammer für den Kreis Mülheim am Rhein (1871–1914) und die Wirtschaft des Köln-Mülheimer Raumes.* (Schriften zur rheinisch-westfälischen Wirtschaftsgeschichte, Number 21.) Cologne: Rheinisch-Westfälischen Wirtschaftsarchiv zu Köln. 1969. Pp. 472.

Like so many other monographs in local German economic history for the period after 1870, this study of the Mülheim chamber of commerce abounds in detail and provides a characteristic example of the current fashion in German "industrial chronology." Thoroughly researched and armed with an extensive bibliography, it claims a multitude of original sources in the town archives of Cologne, Düsseldorf, Porz, and others, as well as the excellent Rhenish-Westfalian economic archive in Cologne, which houses the Mülheim chamber's files. Yet one finds little meaningful emphasis in the selection of material. Despite the impressive listing of archival sources, the actual references are to the published yearbooks of the Mülheim chamber. The topical arrangement of the massive data reflects the organization of the archives, but it seems to impede the critical analysis of the historian. Consequently, any discussion of the importance of the chamber's policies at critical junctures of German history is lost in the endless recitation of facts. The free-trade position of the chamber and its contribution to Wilhelminian tax reforms, for example, are accorded less space than its routine work on weights and measures, enlargement of the harbor, improvement of railway stations, banking and credit, patents, postal services, and roads and trolley cars. Labor relations are given short shrift, and only the opposition of

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the chamber to the consumer cooperatives, which offered keen competition to the wholesale food industry, indicates that any social conflict existed at all.

The history of the Cologne economic region, which precedes the history of the chamber, was intended to provide a frame of reference for the study of the chamber's evolution, but the author merely presents a detailed outline of the growth of the major firms in the Mülheim area. Heuristic principles and concepts of analysis are ignored, and the entire section proves to have been unnecessary to the history of the chamber. The encyclopedic nature of the treatment may be useful for other local researchers, but it can hardly be called history. Only the sketch of the development and the statistical analysis of the cotton textile industry are meaningful and interesting, for this industry shows the increasing pressure to mechanize rapidly in order to maintain a competitive position, while at the same time gaining rather little in increased revenues. It was, however, the interests represented by the dye industry (based on local lead and zinc mining) that tended to favor free trade, for by the end of the century Germany was no longer self-sufficient in raw materials in this, its most famous chemical industry. Yet the Prussian government tended to work for greater protection. Still, the chamber was a semigovernmental institution, its members were elected from the taxpayers' rolls, its elections were supervised by state officials, and the government used, long before the emergence of Hitler's "corporate state," the advisory opinion of the chambers in drawing up legislation.

HELEN P. LIEBEL
University of Alberta

HELMUT HIRSCH, editor. *Eduard Bernsteins Briefwechsel mit Friedrich Engels.* (Quellen und Untersuchungen zur Geschichte der deutschen und österreichischen Arbeiterbewegung, New Series, Number 1.) Assen: Van Gorcum & Comp. N.V. 1970. Pp. xxxvi, 452. 59.50 gls.

Between 1879 and 1895 Eduard Bernstein occupied two important posts for the German Social Democratic party, first as editor of the party's illegal newspaper, published in Switzerland, later as the party's London correspondent; for even after the ban against the party was lifted, he was still threatened by an arrest warrant and could not return to Germany. Particularly in the first of these assignments he sought advice regularly from the two grand old men of the movement, or, rather, since Marx was unapproachable, from Engels. In time, their relationship grew from businesslike cordiality into a warm friendship that lasted until Engels's death. The growth of this friendship can be traced step by step in the correspondence they maintained.

Most of the material contained in the 174 letters printed in the volume under review was published in 1925 by Bernstein, in a well-annotated volume that is now a rarity. Bernstein at that time left out some strictly personal matters as well as sexual references and some of the four-letter words that Engels so liked to use. In the present edition the letters are printed in their entirety; and wherever possible, passages struck out by the writer have been added in parentheses, so that we can observe the writers drafting their letters.

This volume is a work of impeccable and thorough scholarship. Most of Bernstein's notes have been kept, but much further annotation has been added, betraying the editor's profound knowledge of his material and of all the relevant literature, old and recent. In every technical detail this is a pleasing book.

For the student of the Marxist movement, these letters provide interesting glimpses into the thinking of both men, especially with regard to the day-to-day tactics of the movement and to publications policy. It is remarkable to note the rigorous Marxist orthodoxy of Bernstein, which gives no hint of his subsequent revisionism. Engels writes some interesting letters on problems of Irish and Balkan nationalism. His letter of October 9, 1886, will be of interest to students of modernization theory as well as to those interested in the role of the family. Some of his strongest invectives are reserved for revolutionary phrase makers, anarchists, and those impatient to await the gradual development of socialism. "The greatest danger for political emigrés lies in militancy; (*Tatendrang*) something must happen; something must be done! And in this way things happen, the later effects of which cannot be envisaged and which, as the instigators themselves will see later, would better have remained undone.

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Do you . . . perhaps still suffer from a touch of militancy? Then do beware—of yourself." (Engels to Bernstein, Nov. 4, 1882).

ALFRED G. MEYER
University of Michigan, Ann Arbor

PETER-CHRISTIAN WITT. *Die Finanzpolitik des Deutschen Reiches von 1903 bis 1913: Eine Studie zur Innenpolitik des Wilhelminischen Deutschland.* (Historische Studien, Number 415.) Lübeck: Matthiesen Verlag. 1970. Pp. 421. DM 56.

Bismarck's constitution hamstrung imperial taxation in order to preserve states' rights and block "parliamentarism." This is the first documented study of Wilhelmine financial policy, an unusually well-balanced dissertation crowned *summa cum laude* at Hamburg. Witt proceeds from the tariff of 1902 through recession, budget reform, demands for military expansion, money-market fluctuations, government aimlessness under the Bülow Bloc, and on to the "great" financial reform of 1908–09. Bülow's personal position then became hopelessly restricted by the Conservatives' demagogic campaign against inheritance taxes. Party positions froze and the chancellor was destroyed in 1909. In the denouement Witt sees a "consolidation of Imperial finances under . . . Wermuth" and a "militarization of Imperial financial policy" in the expanded Army Bill of 1913 and in the introduction of direct taxation at the imperial level.

Prussian Conservatives feared already before 1908 to "blow up the Bloc" and ditch Bülow, which would increase the probability of concessions to middle-class interests while losing the one personality who in their view could restrain the emperor. The *Daily Telegraph* episode panicked them; a single superficial politician threatened the entire structure of conservative privilege. Yet confusion within the Conservative parties was greater, and the meaning of the possible alternatives was less clear to the principals than often is assumed. Witt criticizes Eschenburg's classic interpretation, recently reinforced by West German writers, which saw in the Bloc the possibility of taking the *Scheideweg*, the fork in the road that would lead through genuine constitutionalism to parliamentary government. The possible alternative to irresponsible cabinet government and "personal regime" was more modest: at best, the Conservatives might "show insight and voluntarily renounce a portion of their privileges," to the benefit not of the entire polity nor even of the middle classes, but in favor of a particular stratum of privileged bourgeois on whom they were dependent in imperial politics outside of Prussia, and thus merely "open the way" for a possible later realization of a limited constitutionalism. When the Conservatives decided "against Bülow, against the National Liberals, and against any material sacrifice and for the Emperor, for the Center, and for the protection of their pocketbook," they took an "indirectly" but "logically" self-defeating course.

What did civilian officials know of military plans prior to war? They supported, after all, "the largest army expansion ever undertaken at any time in Germany" and were told expressly in the Bundesrat that Belgium would be among the enemies in the next war. Financial debates show "to what a high degree the army and finance bills were conceived with a concrete case of war in mind and how unreservedly financial policy was placed [by civilians] in the service of the military machine." Whatever the final judgment on this line of investigation, it is surely more promising than that which ever since Clemens Delbrück has argued that because domestic officials failed to plan a long-term domestic mobilization, they must therefore have had no thoughts of impending war.

Witt, Dirk Stegmann, and Klaus Wernecke were intimately associated with the production of Fritz Fischer's *Krieg der Illusionen* (reviewed *AHR*, 76 [1971]: 791–92). Their works may be viewed as documentary *Unterlagen* for Fischer's more sharply stated themes. Taken with the work of Egmont Zechlin and his students, they are proof, were it still needed, that the Hamburg Historical Seminar has consolidated a major advance in the study of the German Empire.

PAUL R. DUGGAN
Michigan State University

HANS-ERICH VOLKMANN. *Die deutsche Baltikumpolitik zwischen Brest-Litovsk und Compiègne: Ein Beitrag zur "Kriegszieldiskussion."* (Ostmitteleuropa in Vergangenheit und Gegenwart,

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Number 13.) Cologne: Böhlau Verlag. 1970. Pp. 283. DM 68.

I had read relatively few pages of this capable monograph before it suddenly evoked memories of that wonderful third volume of Arnold Zweig's trilogy of the First World War, *The Crowning of a King* (1938). The same Baltic realms are at play here; empires—or at least duchies—at stake; mysteries of high policy about territories whose inhabitants are barely recognizable in the background. More realistically, this work is a thorough and comprehensive study of German policy toward Estonia, Livonia, Courland, and Lithuania in the months from Russia's first peace negotiations to the armistice on the western front. Hans-Erich Volkmann has drawn from a wide collection of primary materials in Bonn, Koblenz, Freiburg, Stuttgart, Munich, and Berlin. The bibliography is extensive. The work appears under the auspices of the J. G. Herder Institut in Marburg.

About half of Volkmann's study is concerned with Brest-Litovsk; the remainder with the policies of Richard von Kühlmann and his successor as foreign minister, Paul von Hintze, toward Courland and the final separation of Livonia and Estonia from Russia in the supplementary treaty of Brest-Litovsk. He concludes with the few frantic weeks of Prince Max of Baden's ministry. With considerable success, Volkmann delineates the shifting fortunes of three lines of German policy: Ludendorff's, Kühlmann's and Hintze's, and the majority parties' of the Reichstag. Volkmann believes that Kühlmann has too often been tarred by the same brush Fritz Fischer and others have used on Ludendorff and the whole problem of German war aims. He sees in the views of Kühlmann and Hintze an awareness of a long line of German Eastern policy, stemming from Bismarck and seeking equilibrium with Russia. He develops very interestingly the concern of the interparty committee of the Reichstag with the concept of self-determination in the East.

Despite the overwhelming pressures of Ludendorff at Brest, Volkmann sees a rise in the influence of Kühlmann and Hintze later in 1918. I suppose one could debate until the cows come home the roles of these men; but here is one of the virtues of this book: it explores details and develops hypotheses. Another contribution is the delineation of the varied opinions about German war aims, which still existed in 1918 despite Ludendorff's monolithic influence. This is a useful book for the more specialized student.

THOMAS T. HELDE
Georgetown University

KARL DIETRICH BRACHER. *The German Dictatorship: The Origins, Structure, and Effects of National Socialism.* Translated from the German by JEAN STEINBERG. With an introduction by PETER GAY. New York: Praeger Publishers. 1970. Pp. xv, 553. $13.95.

This is probably the most important general book on Nazi Germany since Bullock's biography of Hitler. But the reader should be forewarned that, despite its length and chronological arrangement, it is not a comprehensive narrative history of the Third Reich. Instead Bracher provides us with an overarching interpretation of the German dictatorship by concentrating on the three topics in the subtitle—origins, structure, and effects. This accounts for what would otherwise seem to be gross imbalances in coverage. The author assumes a good deal of information on familiar matters (the Blomberg-Fritsch affair, for instance, is disposed of in three sentences), while giving us mini-monographs on topics (education, social change, the SS state) vital to understanding National Socialism. Bracher nevertheless covers the entire waterfront, however unevenly, all the way from unwitting precursors like Fichte to faithful torchbearers like Adolf von Thadden.

As Bracher sees it, National Socialism was an amalgam of two deeply rooted German traditions: the tradition of the bureaucratic authoritarian state standing above both society and the individual, and the *völkisch* tradition of social Darwinian racism, anti-Marxism, and anti-capitalism. Thus the Nazi dictatorship was no mere accident (*Betriebsunfall*) in German history. On the other hand, the two traditions could never have been fused into a mass movement but for the national disasters of defeat, revolution, inflation, and depression. Even then Hitler could never have come to power had not Hindenburg, surrounded by a camarilla intent on destroying the Weimar Republic, been president in 1933. In tracing the rise of

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National Socialism Bracher thus distinguishes carefully between necessary and sufficient causes.

The treatment of the Twelve Year Reich itself is likely to prove more controversial. Rejecting all revisionism, Bracher insists that Hitler never wavered from the key ideas laid down in *Mein Kampf* ("race doctrine and *Lebensraum* theory") and that the implementation of these ideas beginning in 1933 produced, as a logical result, "*Gleichschaltung* and repression internally, expansion and destruction externally." The war did not result from misread signals, nor was it something "forced on" Hitler (the Hoggan thesis); it was an inevitable necessity to the regime. Likewise the Himmler empire was not a freak wartime growth but rather the blueprint of a postwar Hitlerian Europe. In short the so-called excesses of the German dictatorship were in fact its essence, and intentionally so.

Bracher concludes with a very informative analysis of neo-Nazi periodicals and movements in the Bonn Republic—he finds their readership and potential following depressingly large—and with an appeal to left-wing youth to make the second German democracy work instead of calling, as the Nazis did, for the overthrow of the "system."

The English translation is accurate and helpful—German terms are often inserted in brackets. In two respects it is an improvement on the original: it contains an index instead of just a *Personenregister,* and the very full bibliography cites non-German books in the original edition rather than in a subsequent German translation. Minor changes were made in the last chapter to take into account the decline of the NPD since the German edition was published in 1969. Unfortunately there is nothing that Steinberg or any other translator could do about Bracher's style: "It reads itself like leather," as the Germans would say, but it's well worth the chewing.

ROBERT E. NEIL
Oberlin College

INGE SCHOLL. *Students against Tyranny: The Resistance of the White Rose, Munich, 1942–1943.* Translated from the German by ARTHUR R. SCHULTZ. Middletown, Conn.: Wesleyan University Press. 1970. Pp. 160. $5.00.

LAWRENCE D. WALKER. *Hitler Youth and Catholic Youth 1933–1936: A Study in Totalitarian Conquest.* Washington: Catholic University of America Press. 1970. Pp. x, 203. $10.50.

A moving and authenticating feature of both the books under review is the presentation of change in young people when confronted with unusually large contradictions between internalized social goals and values and the real world. Both authors emphasize the slow and painful process of "awakening"—a process in Nazi Germany that was so slow that it came too late to be socially effective—culminating in an inner struggle to stay "within the fold" before finally acknowledging the basic antipathy between religious convictions and the Nazi way of life. While Inge Scholl's naive account of her brother's and sister's little resistance group at Munich University is almost totally microcosmic and subjective, Lawrence Walker's equally brief study strikes a balance between descriptive breadth (German youth movements) and the microanalysis of one Catholic resistance organization, the *Katholischer Jungmännerverband.* In his translation Professor Schultz has caught the pathos and the clarity of both Inge Scholl and the leaflets that the White Rose group distributed. Other documents appended to the narrative include the indictments and sentences (actually the judgments as well) of the conspirators, showing the ambit of the group as well as their accomplishments, along with a moving letter relating the last days of the Scholls in prison. The original text was "written in 1947 for use in German schools for adolescents from the age of thirteen to eighteen" (p. 94). The remainder of the book was added for the American edition, along with some additions to the narrative (not indicated, except for the concluding remarks, which expand slightly the rather weak political interpretation of the main text).

Professor Walker's book, too, suffers from rather ineffectual political analysis—a more serious indictment in view of its scholarly format. The author does not succeed in placing the German youth movements into a social and political context. In fact the reference to Hitler Youth in the title is really justified only in part by some dozen pages of his book (pp. 10–22) based on the Party *Hauptarchiv.* All the rest of the book deals with the evolution of Catholic Youth and their response to Nazi persecution

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and surveillance. The value of his study is its use of unpublished Nazi records to show the persistence of Catholic resistance organizations and his use of Catholic records (these mostly published) to show the long-drawn-out phase of accommodation and adaptation the hierarchy went through as it gradually faced up to Nazi tyranny. The chapter, "Nazi Policy Toward Catholic Church" (*sic*) is the only interpretive chapter in the study; and while it is good, sound exposition of the profound misunderstanding by the Nazis of all religious behavior, it, too, barely scratches the surface of explanation. Walker hints from time to time that the caritative strands of "youth work," which always sought to educate and lead youth into adulthood "within the fold," could not be adapted very skillfully except by the Nazis to twentieth-century youth's new-found political value. Ultimately the Hitler Youth became the worst mockery of *Selbtbehauptung* (self-assertion)—as the Scholls discovered—a more thoroughly (and yet falsely) caritative case of "youth work" than *Pfadfinder* (Boy Scouts) or *Jugendhäuser* (Catholic Boys Clubs). Walker speaks of the Catholic Church capturing the moral zeal and deep sense of community of earlier youth movements and infusing them with religious purposes (p. 159). Here he betrays the limits of his own historical insight; if all that the church could do was "capture" and "infuse," then it had to fail, confronted with practitioners as ruthless and thorough as the inquisitors of the Counter-Reformation. In truth the Christian religion is a living faith and a community today because it has been restored again and again by its martyrs, nourished, it is true, by organizations, but ultimately making their witness alone, as individuals.

ROBERT KOEHL
University of Wisconsin, Madison

RICHARD D. MANDELL. *The Nazi Olympics*. New York: Macmillan Company. 1971. Pp. xvi, 316. $7.95.

This book is both a narrative thriller and a scholarly disappointment. Intended primarily for the general reader, it offers a great deal of entertaining (if not always important) information about the 1936 Olympics. Chapters cover such divergent topics as the contests themselves, personal tidbits about some of the more prominent athletes, and an analytical description of Leni Riefenstahl's film *Olympia*. The book is largely unencumbered by scholarly apparatus (there are footnotes at the back, but no bibliography), and the smooth style sounds only occasionally like the *New Yorker*'s parody of a sports writer. Indeed, as a sports narrative the book is an unqualified entertainment success.

But the author has larger ambitions as well, and there lies the difficulty. He intends to show "that the Olympics were an obscuring layer of shimmering froth on a noxious wave of destiny" (p. xiv). (As noted, there are some stylistic difficulties.) Specifically, Professor Mandell contends that Hitler used the Olympics deliberately to obscure his expansionist plans and that Avery Brundage aided and abetted him. There is some doubt about the ranking of the two villains, though on balance Hitler is the more onerous. The author does, however, hold Brundage responsible for sabotaging a near-successful attempt by the United States to boycott the Berlin games and thereby to spoil Hitler's triumphs.

There is little doubt, of course, that the Nazis used the Olympics quite consciously to bolster the image of a peaceful, efficient, and powerful Germany. What is disappointing, almost frightening, about Professor Mandell's book is his use of near demagogic methodology to obscure perfunctory scholarship. The book is rather short on documentary digging. The author claims, for example, that the German athletes performed well at Berlin only because they were in the "psychic grip" (p. 288) of Nazi totalitarianism. Perhaps. But it certainly helped that an athlete like Karl Hein (the gold medal winner in the hammer throw) was able to attend training camps on his employer's time. More disturbing still is the author's willingness to substitute heart-wrenching for solid scholarship. He is very adept at the use of stereotypes; the Germans in the book would look quite comfortable on the set of *Hogan's Heroes*. They even laugh and tickle gutturally (p. 157), and their humor "is inevitably puzzling west of the Rhine" (p. 265). Above all, however, the author's presentation relies upon the present-day reader's knowledge of and emo-

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tional recoil from the horrors that followed from Nazi actions after the Olympics. Thus Professor Mandell reminds us that while Hitler may have been present in the stadium in August 1936, his mind was really concerned with the conquest of Czechoslovakia two years later. This sort of statement, of course, cannot be documented; its acceptance depends upon the reader's benefit of hindsight. Unfortunately, such methodological sleight-of-hand also cheapens what is in many ways an admirable account and significantly detracts from its usefulness as a work of serious historical scholarship.

DIETRICH ORLOW
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HELMUTH GROSCURTH. *Tagebücher eines Abwehroffiziers 1938–1940: Mit weiteren Dokumenten zur Militäropposition gegen Hitler.* Edited by HELMUT KRAUSNICK and HAROLD C. DEUTSCH, with the assistance of HILDEGARD VON KOTZE. (Quellen und Darstellung zur Zeitgeschichte, Number 19.) Stuttgart: Deutsche Verlags-Anstalt. 1970. Pp. 594. DM 38.

This massive and well-edited volume of documents contains Groscurth's private and official diaries from August 1938 through February 1940, numerous secret documents that came across his desk, some previously unpublished memorandums by members of the German military resistance to Hitler, and letters to and by Groscurth, 1940–43. The unusual significance of this source collection, which, until now, was little known and less used, derives from the positions held by Lieutenant-Colonel Groscurth. During 1938 he headed Section II of Canaris's *Abwehr* and was in charge, among other things, of maintaining liaison with German and other ethnic minorities—for example Henlein's Sudeten Germans—in various European countries. From August 1938 through February 1940 he led a special unit (*Abteilung z.b.V.*) of the German general staff where he dealt with questions relating to foreign and domestic policy, propaganda, army morale, army frictions with the NSDAP and the SS, and others. Thus exceptionally well informed, he used his positions to participate very actively in the military opposition to Hitler, maintaining contacts with Halder, Beck, Oster, and others, and agitating with such insistence that he was dismissed from his post in 1940. But the importance of these documents goes beyond the detailed description of the activities and personalities of military leaders opposed to the Nazi regime and the coming war in 1939. They also show the beginning of the submission of Germany's military leadership to the criminal political leaders of the Third Reich and their increasingly helpless acquiescence in the atrocities of the SS. They also provide a detailed picture of the German take-over of the Sudeten area, of Hitler's unflinching determination to destroy Czechoslovakia and to unleash war, and of his cynical dealings with friend and foe alike in matters of foreign policy. And finally there emerges from these pages the image of a nationalist, conservative, "Prussian" staff officer whose moral revulsion and professional ethos led him into active, if finally ineffectual, opposition to Hitler's regime. This collection is, therefore, of major interest to specialists in several areas of recent German history. An excellent introduction by the editors, copious and informative footnotes, bibliography, and an index of persons provide valuable aids to the user.

HERBERT A. ARNOLD
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JERZY KRASUSKI. *Podział Niemiec NRD i NRF w Latach 1949–1955* [The Division of Germany into the German Democratic Republic and the German Federal Republic, 1949–1955]. (Studium Niemcoznawcze Instytutu Zachodniego, Number 18.) Poznań: Instytut Zachodni. 1969. Pp. 200. Zł. 30.

An occupational hazard connected with the writing of contemporary history—the risk of having an interpretation vitiated by the impact of subsequent events—is strikingly evident in this book. The author, a prolific specialist on German-Polish relations, bases his study entirely on published materials, mostly Keesing's *Contemporary Archives* and an assortment of German works.

Familiar landmarks dot the narrative landscape, although little analysis is given to the German Democratic Republic. The international relations of the era between the creation of two German states and their subsequent attainment of sovereignty are examined to demonstrate that German unity became both a pawn and a victim of the cold war. Krasuski

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concludes that the West deliberately fostered German duality for cold war ends and that Konrad Adenauer encouraged the process by committing West Germany to European integration in hopes of obtaining greater support for reunification and the recovery of territory lost at Potsdam. Because his "sponsors" desired it, Adenauer promoted remilitarization, which ruined the prospects for a unified, democratic, and peaceful Germany envisioned and championed by the Soviet Union. The USSR could not allow German militarism, acting as a tool of Western capitalism, to threaten the peace again. Hence, for defensive reasons the Soviets sought to safeguard socialist gains in the GDR and Eastern Europe.

Throughout the study Krasuski not only castigates the West—especially the United States—and Adenauer, but he also criticizes the SPD for its agreement on the ends if not the tactics of Adenauer's reunification policy. His interpretation is obviously not unique; the book is yet another rendition of the familiar refrain played by Polish scholars until recently. The West German–Polish agreement reached earlier this year undermines the author's contention that Bonn adheres to a policy bent on eliminating East Germany and also recovering the eastern territories. Among the consequences emanating from this agreement will, I hope, be a scholarly climate wherein this book rapidly acquires "period-piece" status.

LARRY V. THOMPSON
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RICHARD GEORG PLASCHKA and KARLHEINZ MACK, editors. *Die Auflösung des Habsburgerreiches: Zusammenbruch und Neuorientierung im Donauraum.* (Schriftenreihe des Österreichischen Ost- und Südosteuropa-Instituts, Number 3.) Vienna: Verlag für Geschichte und Politik. 1970. Pp. 556. Sch. 340.

This is a collection of sixty-one papers originally presented at an international symposium held in 1968 in Vienna under the auspices of the Austrian Ost- und Südosteuropa Institut, a private foundation. The Austrian government has provided a subsidy for the publication of the papers. The purpose of the symposium was to re-examine the immediate circumstances in which the Danubian monarchy broke up in 1918 and the Succession States came into existence.

The unwieldy number of contributors, the extremely diverse character of the subjects covered, and the widely differing length, scholarly apparatus, and quality of the articles make it difficult to assess the general quality of this symposium. Some of the contributions are extremely good and others are worthless, some offer new information, others are discussions of already known fact, some deal with minute details, others are broad surveys, some emphasize political history, others are sociological. From such diversity it would be unfair to expect any new comprehensive interpretation of the collapse of the monarchy and the shape of the new order in East Central Europe. Taken together these articles reinforce the accepted picture of interaction between external and internal developments that, coming to a climax in 1918, made the dissolution of the multinational state an irreversible fact. No new facts emerge here that would force us to change our idea of what happened or why.

Many of the articles here are nevertheless extremely valuable individually. A few that may be mentioned are R. A. Kann's analysis of the peculiar character of the Austrian constitutional system and its influence on the course of events, R. John Rath's historiographical essay, and Fritz Fellner's erudite contribution on the far-reaching historical importance of the Empire's dissolution.

The fact that forty of the contributors here are from Communist-ruled states raises again the tedious but insistent question of what valid contribution to historical knowledge can be made by Communist scholarship. On the basis of these articles we can observe that the treatment of the Russian October Revolution as a sacred event of transcendent importance is tedious and distorting; the concept of imperialism is narrow and cliché-bound; the premise that ideological movements represent only the superstructure of the class struggle is unacceptable. The editors in their introduction lavish praise on the objectivity that these contributions show is now at last possible, but most of the writers from the Eastern European states have in fact merely shifted the emphasis from the older concept of politico-national oppression to economic and social oppression. But the villain is the same: the old Habsburg establishment, whether denominated as "German," "Magyár," "capitalist," or "feudal." This is a

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fundamentally unhistorical perspective and a retrograde development in scholarship. The picture of a social and economic *Völkerkerker* that many contributors present is grossly overdrawn and in the light of recent history naive or disingenuous.

We do not, however, have to give up all hope of fruitful contributions from Eastern Europe. Fortunately some of the scholars represented here, after the obligatory bow to Lenin, reveal interesting new information gleaned from archives about strikes, revolutionary committees, statesmen's decisions, and like details that are unimpeachable contributions to knowledge. The essay by Peter Hanak of Budapest on public opinion as revealed in the archives of the wartime censorship is a model of this kind. The general bias of Marxism may also perhaps be credited with leading to greater appreciation of sociological analysis of the events preceding the dissolution of the Empire. These essays in general, it must be said, reinforce the pessimistic opinion of Western scholars at recent international historical congresses that it is probably impossible to achieve a satisfactory synthesis of the complex process of dissolution of the Habsburg Empire using the Marxist-Leninist historical concept.

Forty years ago Oscar Jászi in his *Dissolution of the Habsburg Monarchy* (1929) confessed to despair that any one man could ever comprehend every facet of that process. This international work of multiple authorship does not offer much hope that the method employed here will be notably more successful. What this symposium offers the specialist is a mixed bag of special studies, some of which are extremely interesting and valuable.

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PETER ADAM THRASHER. *Pasquale Paoli: An Enlightened Hero, 1725–1807.* [Hamden, Conn.:] Archon Books. 1970. Pp. 352. $9.00.

Pasquale Paoli was a Corsican who in 1755 was called by his fellow islanders to lead Corsica's revolt against the Republic of Genoa. Mr. Thrasher presents him as a "child of the Enlightenment." The son of an early leader of the Corsican revolt who had to go into exile in Naples, he spent his formative years in the Naples of Charles III, Tanucci, and Genovesi where he pursued a military career to support himself. He spoke and wrote of reason and utility; as the leader of Corsica he not only fought for Corsican independence but issued a constitution providing for universal suffrage. After many years of exile in England—which began after Genoa ceded Corsica to France in 1768—he agreed to return to Corsica and to govern it during the French Revolution. For his constitution and "enlightened" rule in general he received a certain amount of acclaim from the French *philosophes.*

Mr. Thrasher traces Paoli's career from his Naples days on. After returning to Corsica under the aegis of the Revolution, Paoli found that young Corsicans regarded him as too moderate. Napoleon Bonaparte and his brothers turned against him as did Cristoforo Saliceti, who was connected with the Jacobins. Denounced to and condemned by the Convention, Paoli sought assistance from the English, and, hoping that Corsica would be accorded autonomy under the English Crown, he admitted the English to Corsica and allowed the formation of an Anglo-Corsican kingdom. This state lasted from 1793 to 1796, when, with revolt underway, the British abandoned Corsica and the French returned.

Thrasher's study of Paoli is not the first that has been made, and I regret to say that it is not a very good one. It is not scholarly, and not too good as "popular" history either. A civil engineer (though he has a degree in history), Thrasher lacks sufficient historical knowledge—especially of the French Revolution—and historical judgment. He exaggerates Paoli's importance and the acclaim he received. He misunderstands the Jacobins; and to him Paoli's opponents, whether the Genoese or the Jacobins, are bad people. But he is good on some things: the nature of Corsican society; the infatuation Boswell developed for Paoli; and the British viceroy of Corsica, Sir Gilbert Elliot. Thrasher shows very well how Gilbert, like Burke, believed that the British constitution and governmental system were perfect and how he sought to make Corsica over on the model of England.

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SALVATORE SECHI. *Dopoguerra e Fascismo in Sardegna: Il Movimento Autonomistico nella Crisi dello Stato Liberale (1918–1926).* ("Studi," Number 8.) Turin: Fondazione Luigi Einaudi. 1969. Pp. 504. L. 5,000.

Immediately after the end of World War II, which brought about the fall of fascism in Italy, Italian historiography on fascism was dominated by the writings of men such as Gaetano Salvemini, Giuseppe Borgese, Luigi Salvatorelli, Giovanni Mira, Angelo Tasca, Guido Dorso, Leo Valiani, Ernesto Rossi, Nino Valeri, and many others, who wrote from personal and often painful experiences with the regime. Their works remain important and valuable, but as the fascism that arose in 1919 slips into past history, a re-evaluation of its origins in and relation to Italy's historical development is being attempted by younger historians, many of whom know fascism only as a phase in Italy's past.

An ideological debate, however, on the basic question of why fascism took root in Italy continues, as De Felice clearly summarizes and analyzes in *Le interpretazioni del fascismo* (1969) and *Il fascismo, le interpretazioni dei contemporanei e degli storici* (1970). These ideological interpretations have tended to becloud understanding of the fascist phenomenon rather than illumine it. But as Sechi comments, earlier studies now begin to appear passé and not very significant, precisely because of the ideological and political position of the authors who were often protagonists in the events described. There is need, he writes, "to overcome this limitation in [Italian] historiography and to start anew, studying the fascist movement . . . region by region, so as to grasp its specific quality" (p. 320, fn. 110).

Written as a doctoral dissertation, this book suffers from overdetailed presentation and some repetitiousness, but it is thoroughly documented by references to archival material, newspapers of the times, and important secondary sources. Not a work for the general student of fascism, it should be useful for those who wish to understand how fascism was able to overcome opposition and assert itself in a region of Italy where its appeal was minimal. In discussing the rise and establishment of fascism in the crucial years 1919–25, Sechi carefully studies Sardinian political, social, and economic conditions. Since unification Rome had disregarded the island's poverty and backwardness. In 1918 Sardinians demanded solutions to these age-old problems. Perhaps with some exaggeration many of them called their island the Italian Ireland and pressed for regional autonomy, a demand locally more appealing than fascist slogans. By April 1921 fascism numbered only a few hundred members, drawn mainly from veterans and students, but as fascism gained power on mainland Italy, its following in Sardinia grew. Civil strife ensued and the black shirts fought Lussu's "grey" shirted followers. After October 1922 opposition to fascism became better organized. At the same time fascist ranks swelled, many joining what now appeared to be the stronger political force. By 1925 opposition had disintegrated and fascism was in control.

For me the real value of the book lies more in its analysis of local rivalries, problems, and shortcomings than in its description of fascism in Sardinia. Thus, while Sechi's account of Sardinian conditions is valuable, it does not point the way to a reinterpretation of fascism. It supplies, however, an analysis, *in parvo,* that enriches our growing understanding of why liberal Italy ushered in fascism.

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LAZAR VOLIN. *A Century of Russian Agriculture: From Alexander II to Khrushchev.* (Russian Research Center Studies, Number 63.) Cambridge, Mass.: Harvard University Press. 1970. Pp. viii, 644. $18.50.

D. J. MALE. *Russian Peasant Organization before Collectivisation: A Study of Commune and Gathering 1925–1930.* (Soviet and East European Studies.) New York: Cambridge University Press. 1971. Pp. viii, 253. $12.50.

The posthumous publication of Lazar Volin's admirable survey of Russian agriculture from the emancipation of the serfs to the end of the Khrushchev era brings a major contribution to the field. A leading authority on the subject, Volin was a Soviet specialist in the U.S. Department of Agriculture until shortly before his death in 1966. To this final enterprise he brought those qualities of mind and learning that conspire to make a success of such an ambitious undertaking: fairness of judgment,

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broad historical and comparative perspective, personal observation in the field, and sound technical training.

It should be emphasized that this is no pastiche assembled by friends and colleagues, nor a text abruptly punctuated by the author's death. In all major respects it is a finished work. Well planned and competently executed, it provides comprehensive coverage of the topic.

To call it a history of Russian agriculture would be misleading. It is better described in the author's own words as "a systematic account of contemporary collectivized agriculture, its structure and the manner in which it functions or malfunctions," preceded by a historical review. The description is justified both by the organization of this big book and in terms of its approach. Part 3, "Collectivized Agriculture under Khrushchev," covers 230 pages, considerably more than either of the earlier sections dealing respectively with the pre-Revolutionary period and the years 1917–53. The historical review is built on a crisp chronological narrative of agrarian policies and politics, while structural analysis and agriculture proper are left largely to the Khrushchev era.

The result is a coherent picture of the nature and persistence of the perennial "agrarian problem." Historical perspective gives emphasis to the author's case for strong lines of continuity in peasant-state relationships, in the recurrence of behavioral patterns, and in the chronic struggle with natural and climatic limitations.

Despite his acknowledgment of the more egregious miscalculations of Khrushchev's agricultural policies ("maize mania," excesses of the virgin lands program, support of Lysenko), Volin presents a generous and persuasively reasoned assessment of the energetic and pragmatic leader. Compared to the Stalin era, agricultural policy under Khrushchev displayed, on the whole, an increment of economic rationality, and Volin shows that it bore fruit in increased peasant income and agricultural output.

The economic implications of extreme political and administrative centralization, obsession with gigantism in agriculture, peasant incentives, and investment priorities are among the issues subjected to perceptive analysis. Volin's prognosis for Soviet agriculture is far from sanguine, but the possibility of a modest and gradual improvement in the peasant's position is hopefully advanced.

To the extent that the book deals with contemporary agriculture the four-year publication delay is regrettable, and it is a pity that so valuable a work was not provided with a more adequate index.

Growing interest in the rural scene in the critical interval between the Revolution and collectivization is reflected in Donald Male's study of structural aspects of peasant life in the late 1920s. Despite prewar policies, and without official support, the commune showed a remarkable resurgence after the Revolution; communal tenure predominated strongly throughout the following decade. Male's book is an attempt to describe some of the characteristics and functions of the system, to sketch the bare bones of the institutional and administrative apparatus of the countryside. The relations of commune and rural soviet are indicated, and a noteworthy appendix grapples valiantly, though inconclusively, with the question of the physical relationship between the commune and the succeeding collective farm.

In view of regional variations and formidable source problems the author's cautious approach to generalization is well warranted; the use of census data, however, would have permitted more precise quantification. This descriptive skeleton calls for historical and analytical flesh but is a welcome addition to the scant material in an important and long-neglected area.

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TERENCE EMMONS
Stanford University

A. L. NAROCHNITSKII *et al.*, editors. *Vneshniaia politika Rossii XIX i nachala XX veka: Dokumenty Rossiiskogo Ministerstva Inostrannykh Del* [Russia's Foreign Policy in the 19th and Early 20th Century: Documents from the Russian Ministry of Foreign Affairs]. First Series, *1801–1815 gg.* Volume 7, *Ianvar' 1813 g.–mai 1814 g.* [January 1813–May 1814]. (Ministerstvo Inostrannykh Del SSSR.) Moscow: Izdatel'stvo Politicheskoi Literatury. 1970. Pp. 870.

The first six volumes of this selection of documents from the Russian Ministry of Foreign Affairs have been reviewed in this journal (See

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AHR, 67 [1961–62]: 1110; 68 [1962–63]: 1136; 69 [1963–64]: 456; 72 [1966–67]: 647; 75 [1969–70]: 170). Previous reviews have praised the format adopted by the editors, a group of distinguished Soviet scholars. The same high standard is maintained in the seventh volume, the heart of which is the 280 previously unpublished documents—letters, dispatches, instructions, and the like—followed in most cases by approximately two hundred supporting documents that have been published before in various works but are here conveniently grouped with related materials. The value of these primary sources is considerably increased by detailed explanatory notes (running to ninety-seven pages of fine print) containing important bibliographic citations, name, subject, and geographic indexes, and a short but useful bibliography.

Like earlier volumes, the seventh is an attempt to illustrate the full range of Russian foreign policy. Therefore, documents are included on relations, *inter alia*, with the United States, Latin America, the Ottoman Empire, and Persia, as well as a few on the Far East. But, at this juncture, these areas were of peripheral importance. The epicenter of Russia's foreign policy, of course, was a wounded but still dangerous France. And the documents in this volume reflect this preoccupation: most of them bear, directly or indirectly, on the crumbling Napoleonic Empire.

Our knowledge of these profound events is reinforced and refined by the evidence here presented. Fresh insights are gained into the forging of the sixth coalition, especially Russia's alliances with Prussia and a much more reluctant Austria; Alexander's plans for Poland and Greece; financing the "liberation of Europe"; and the First Peace of Paris, to name but a few of the significant threads in this complex tapestry. But the documents offer no startling revelations, and they are certainly too selective to provide adequate ground for any major reinterpretation. The editors assert that the evidence in this volume should dispel the "legend" that Metternich "ruled the destinies" of Europe in 1813–14. But that myth has long been punctured. Yet this series is an important addition to the vast literature on European diplomacy of that period. Most of the documents are in French so they are accessible to readers who do not know Russian. And for Western specialists in Russian history, with few exceptions, this collection, along with such standard sources as Martens's *Recueil des traités et conventions conclus par la Russie avec les puissances* (1874–1909) and *Sbornik imperatorskogo russkogo istoricheskogo obshchestva* (1867–1917) will have to suffice until the Soviet government gives foreign scholars free access to the Archives of the Foreign Policy of Russia. It may be a long wait.

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A. J. BARKER. *The War against Russia, 1854–1856*. New York: Holt, Rinehart and Winston. 1970. Pp. xvii, 348. $7.95.

Only mankind's apparently insatiable appetite for bad news can account for "popular" histories of conflicts like the Crimean War. It has been clear for a century that most of the British and French politicians and military commanders, assorted journalists of the David Urquhart stripe, and the gendarme himself, Nicholas I, richly deserved to be bound over to the criminal assizes. They were not, justice in these trifles being what it was, but the dismal catalog of their misdeeds has condemned them in the court of history.

The indictment has been available for three generations, and guides to it have been published by the score. This new one, by a British soldier-scholar, A. J. Barker, unfortunately adds nothing to our knowledge. Despite the unusually misleading title, the book constitutes straightforward, by-the-numbers military history. Some sections on the heroism and privations of the allied troops present adequate summaries; the Russian side is sadly neglected (Barker has not even used Tolstoy). We have here the story of who was on whose flank when, of weaponry, gallantry, lunacy, and gore. But it has all been told so many times.

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V. R. LEIKINA-SVIRSKAIA. *Intelligentsiia v Rossii vo vtoroi polovine XIX veka* [The Intelligentsia in Russia in the Second Half of the Nineteenth Century]. Moscow: Izdatel'stvo "Mysl'." 1971. Pp. 368.

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This study of the Russian intelligentsia is in fact a survey of the development of a broad stratum of professional groups in Russian society between 1850 and 1900. The author, a retired researcher in the Leningrad branch of the Academy of Sciences and former scholar attached to the Museum of the Russian Revolution, has published numerous books and articles on the Russian revolutionary movement in the mid-nineteenth century. Her new book marks the first attempt by a Soviet scholar to explore the social and economic position of professionals in Russian society in the period when these groups first appeared in large numbers.

The first eight chapters are devoted to the development of professional groups. The author focuses attention on technical personnel, the medical profession, teachers and scholars, the literary profession, and specialists within the state apparatus (including the officer corps and Church hierarchy). The scope of her subject is so large that she can discuss individual groups only in a very selective manner. She is interested above all in the educational formation, the range of activities, and the living conditions of these groups. The material, much of which is quantitative, is judiciously chosen and for the most part will be new to Western readers. The only Western works covering a similar subject are certain essays in Cyril Black's *The Transformation of Russian Society* (1961). Leikina-Svirskaia has made a thorough survey of the relevant sources available in the major Soviet libraries. As a result her footnotes provide a good bibliographical guide to the educational and social history of the period.

The rise of professionally trained groups produced an important restructuring of the Russian social hierarchy. Memoirs of the time reveal how different this new elite was from the old noble-bureaucratic elite, and its impact on society was thus inherently disruptive. The Soviet Marxist interpretation, which the author defends, goes one step further. Among the "functions" of the intelligentsia in capitalist societies (including Russia in the late nineteenth century), the Marxist interpretation includes support for the revolutionary movement. It assumes that this political involvement was as intrinsic in the "social role" of the intelligentsia as were those of its activities that supported the class regime. Such a convenient jumbling of political convictions and occupational functions permits the author to stress the participation of professionals in the radical agitation of the period. The last two chapters are entirely devoted to the "educational activities" that spread socialist ideas and to the "professional revolutionaries" from educated classes. This discussion has only a remote connection with the earlier survey of the development of professionals. It does provide convenient support for the Soviet view that the revolution was the inevitable product of Russian social development. But since this view already has the force of dogma in Soviet historiography, the author's evidence is actually superfluous.

An issue more directly relevant to the formation of professional groups is the problem of the involvement of the autocracy in social change. The state in Russia was directly responsible for the training and frequently the hiring of professionals. Their development in both numbers and quality is one indication of the success or failure of the regime to meet the needs of a modernizing society. The author is frequently critical of the efforts of the tsarist regime. She cites the fact that primary-school teachers received miserable wages as "vivid evidence of the backwardness of autocratic power." Yet when measured against the situation in the early nineteenth century, the country had by 1900 made remarkable progress. Comparative data drawn from other countries, such as Austria-Hungary, at a roughly similar point of development, would have been helpful in this respect. The author has written a valuable reference work on the history of professional groups in tsarist Russia while leaving the conclusions to the reader.

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V. O. KLIUCHEVSKII. *Pis'ma. Dnevniki. Aforizmy i mysli ob istorii* [Letters, Diaries, Aphorisms, and Reflections on History]. Edited by M. V. NECHKINA. (Akademiia Nauk SSSR, Institut Istorii.) Moscow: Izdatel'stvo Nauka. 1968. Pp. 524.

Of all the "old guard" historians Kliuchevskii continues to occupy a most honorable place in Soviet historiography. Though often criticized he has seldom been ignored; though frequently

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castigated he still commands the domineering heights in the field of Russian history. He has been accused of "bourgeois views" (mitigated by the qualification of "liberal bourgeois"); he has been charged with conservative proclivities proven by the eulogy he wrote at the death of Alexander III in 1894; he has been censured for membership in the Kadet party and other ideological transgressions. Still the works of Kliuchevskii continue to be published and read widely. Partly it is because of Kliuchevskii's superbly lucid literary style, partly because of his admirable even if debatable interpretation of the *Course of Russian History* (latest publication Moscow, 1956–58). As late as 1946 one Soviet historian, A. I. Iakovlev, urged students to "go back to Kliuchevskii!" thirty-five years (and what years!) after his death. N. L. Rubinshtein, in his presently banned *Historiography* (1941), devoted an entire chapter to Kliuchevskii. If Soloviev is sometimes referred to as the comet on the horizon of nineteenth-century Russian historiography, Kliuchevskii, his pupil and successor, can be rightly considered the star.

Before us is at last a publication long wanted: *Letters, Diaries, Aphorisms, and Reflections on History* of Kliuchevskii. Although it is by no means complete and while a few of the letters have previously appeared in print, this is a most welcome publication. It includes, besides philosophical comments, 120 letters of Kliuchevskii to his friends and members of the family between 1861 and 1911. Cumulatively the publication shows the methodical, meticulous mind of Kliuchevskii as well as the extraordinary effort he always exerted in all his writings.

His early training at the Penza Seminary must have left an indelible imprint on him, even though he dropped out of the institution. He repeatedly stressed, for instance, the importance of Greek and Latin as indispensable tools of historians. Ritualism he considered as necessary as "ashes for the preservation of spiritual warmth against the chills of life" (p. 353). Disheartened occasionally by intellectuals, he compared them to "salesmen offering candy to starving people." Occasionally political climate resulted in desolated feelings. On such occasions Kliuchevskii wrote: "Our future promises to be more oppressive than the past and more hollow than the present" (p. 335). The arbitrary rule of Peter I, Kliuchevskii believed, was at least a chance to lead to freedom, whereas the liberalism of his successors only led to lawlessness (p. 388).

The present volume, though it incorporates only a portion of his correspondence, casts much light on the personality of Kliuchevskii. For this if for no other reason this volume, it is hoped, will be followed by additional ones. Meanwhile, considering the popularity of Kliuchevskii and the scarcity of material related to the development of his personality, the volume reviewed presents an indispensable source to the student of Russian historiography.

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OLEH S. FEDYSHYN, *Germany's Drive to the East and the Ukrainian Revolution, 1917–1918*. New Brunswick: Rutgers University Press. 1971. Pp. xii, 401. $15.00.

While there are three excellent studies of the Ukraine during World War I—John S. Reshetar, *The Ukrainian Revolution* (1952); Jurij Borys, *The Russian Communist Party and the Sovietization of Ukraine* (1960), and Arthur Adams, *Bolsheviks in the Ukraine* (1963)—none devotes much attention to the crucial factor of German policy. The first third of the present volume is devoted to a detailed analysis of German (and, to a lesser degree, Austro-Hungarian) policy prior to occupation of the Ukraine. Professor Fedyshyn joins the numerous scholars who have taken strong issue with Fritz Fischer's famous thesis that German policy followed a long-rang plan for expansion. On the contrary, Fedyshyn concludes, the separate German-Ukrainian treaty at Brest-Litovsk was "not a corollary of a long and clearly discernible process of German political planning and scheming but merely a product of a series of military and economic considerations produced by a long period of ceaseless warfare . . . the beginning of an experiment, undertaken somewhat reluctantly by the Germans, aimed at extending the Reich's influence and power into vast areas of Eastern Europe."

As the author recognizes, his conclusion implies that the focus of German policy evolution prior to 1918 was not the Ukraine but the Baltic—particularly the Ober-Ost region—where

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major figures like Ludendorff began their "education" in Eastern affairs. It is understandable, therefore, that Fedyshyn devotes two-thirds of his book to the post-Brest period when the Ukraine's strategic location and economic potential brought it to center stage. Here his broad use of Russian and Ukrainian materials, as well as the files of the Auswärtiges Amt, gives his book a new dimension. Unfortunately he does not really do much to clear up several important obscurities, particularly in German relations to the Hetman Pavel Skoropadskyi. As Fedyshyn notes, any examination of this question is hampered by the continued unavailability of Skoropadskyi's papers. Nevertheless much more could be done—or so it seems to me—to study the background of the Hetman's supporters and the evolution of their thinking after they had been installed in power. Fedyshyn throws out some useful hints on these subjects, but his conclusion that Skoropadskyi "remained a true Ukrainian patriot to the end of his days" is hardly the last word. If the Hetman movement had been confined to landowners and a few intellectuals further exploration might be unnecessary; the fact that it began with notable peasant support, in the Ukrainian Free Cossacks, for example, and seems to have resulted in the genuine conversion to Ukrainian nationalism of several important tsarist officers makes one more detailed study of the Ukraine during World War I desirable.

JOHN A. ARMSTRONG
University of Wisconsin, Madison

VICTOR PETERS. *Nestor Makhno: The Life of an Anarchist.* Winnipeg: Echo Books. 1970. Pp. 133. $3.75.

Professor Peters has written an objective and readable account of the controversial anarchist leader Nestor Makhno, whose activities in the Ukraine contributed to the defeat of the White armies. The author has gone beyond the recollections of Makhno and his follower Peter Arshinov by obtaining from obscure witnesses reports of those years of violence. Some of these stories are from those who personally experienced the activities of the Makhnovtse, and while Peters brings out the courageous and magnetic aspects of Makhno's personality, the reader is left with a feeling of distaste for his ruthlessness.

The author shows both the extent and the limitations of Makhno's military efforts; for example, the primitive nature of his medical services was truly horrifying. Peters corrects a commonly held notion that Makhno was once a schoolteacher and denies he was anti-Semitic. The book is not, however, based on extensive scholarly research into the history of the revolutionary movement in the Ukraine. Few Russian sources are used. There are errors in the text—for example "Frunze" appears once as "Frunse" and "Bolshevik" as "Bolschevik."

FRANKLIN A. WALKER
Loyola University, Chicago

JOHN BRADLEY. *Allied Intervention in Russia.* New York: Basic Books. 1968. Pp. xix, 251. $6.50.

New studies of the Allied intervention in Russia during and after the First World War inescapably invite comparison with a considerable and interesting collection of books dealing with some aspect of the subject. They also raise the question as to what yet another book will add to our understanding of this complex episode. Professor Bradley's book was apparently intended to be a general study of the intervention with emphasis on the European theater of operations up to 1920. While the Siberian aspects are correctly identified with the events in the West and their beginnings discussed, their projection beyond 1920 receives little attention.

Along with centering his attention on the West, the author also portrays the intervention as a projection of the strategy and hostilities of the First World War. Up to the armistice he sees it as part of the struggle against the Central Powers; after that event the expectations aroused in and concerning Eastern Europe by the defeat of the Central Powers claim the author's attention. In fact, it is the latter aspect of the story that constitutes his best contribution to the literature of the intervention. For he has written a brief but clear statement of the Polish role in the episode as well as one of the best descriptions of the origins of the Czechs' participation and their interests in the affair that has appeared anywhere in the litera-

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ture on the intervention. These chapters are based on archival materials from British, French, Austro-Hungarian, Czech, and Polish files as well as materials from Russian and other sources. Though he deals with the importance for Britain of a Japanese military intervention, he gives comparatively little emphasis to the Siberian events except those in which the Czechs participated and even less attention to the Central Asian-Middle Eastern strategy.

Bradley's emphasis on the interest and role of individual nations in the intervention adds a further argument in combatting the more simplistic view, held by Soviet writers and others, that the Allies generally presented a united front on questions dealing with Russia. In fact, he points out that Allied disunity was a major factor in the failure of the intervention. Far from arising out of a common determination to unseat the new Soviet government, the intervention was the outcome of a general realization of the advantages a Russian departure from the war would have for Germany and the impact this would have on the fortunes of those nations that saw an enhancement of German power as detrimental to their interests. The author's very emphasis on the national interests of the Czechs and Poles, however, means that other factors have been less satisfactorily dealt with and leaves the book something less than the general account the format would indicate. It is regrettable, also, that the author alludes to the special role of the national minorities in the Allied hopes and plans but writes little about this. Finally, better editing and a general index would have improved the formal attributes of an interesting and informative account.

JOHN A. WHITE
University of Hawaii

NEAR EAST

RICHARD ALOUCHE. *Évolution d'un centre de villégiature au Liban (Broummana).* (Publications du Centre Culturel Universitaire. Hommes et Sociétés du Proche-Orient, Number 2.) Beirut: Dar El-Machreq Éditeurs; distrib. by Librairie Orientale, Beirut. 1970. Pp. 312.

This lucid monograph should be welcomed by students of social change in developing countries as well as by specialists on the Middle East. While the Lebanese Christian town of Broummana is of scant historical interest, it becomes, through Alouche's astute analysis, a fascinating microcosm of the development process.

Situated twenty kilometers above Beirut, Broummana has been transformed from a traditional, rural, nuclear village into one of Lebanon's most popular resort towns. Between 1860 and 1925 its character was changed by the abolition of feudalism, the construction of a main road from Beirut, and the introduction of an infrastructure for modernization, notably the Quaker-founded Broummana High School.

The decline of Broummana's traditional economy and society was hastened by the opening of the Suez Canal in 1869, which destroyed the town's silk industry, by various agricultural disasters, and by the calamitous Turkish occupation during World War I. But these events also cleared the way for the development of a service economy. An analysis of the hotel industry shows marked growth with the establishment of the French Mandate in 1920 and even sharper expansion in the post-World War II period. Therefore, by the middle of the 1960s Broummana had become a *village-en-ligne,* with twenty-seven hotels, its mountain character giving way to the ambience of Beirut and Paris. In winter the permanent residents (approximately seventeen hundred) constitute some 80 per cent of the total population; in summer they comprise only 20 per cent.

Alouche's application of spatial as well as temporal concepts of development is intriguing, particularly his analysis of the growth of sectors of the *village-en-ligne.* He shows ingenuity in his interdisciplinary methodology and a keen eye for clues to the development process. Lebanon is poor in statistics, yet the author has pieced together a quantitative picture of the town's transformation. He has also made careful use of a questionnaire survey to discover changing attitudes. His effort to trace class structure and social mobility is interesting, and his analyses of names of quarters, aerial photographs, and the alterations in traditional architecture are especially fruitful.

The author's findings on political apathy and cynicism among Broummanites are significant. The picture he presents of the young Broummanite is familiar and not entirely flat-

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tering: he discovers a new sense of materialism combined with a persistent religious prejudice. The transformation is skillfully accented with an appendix that describes in compassionate detail the resistance of Bou Brahim, the only completely traditional man left in the town, to what he sees as the growing decadence around him.

MICHAEL C. HUDSON
Johns Hopkins University

ROBERT M. HADDAD. *Syrian Christians in Muslim Society: An Interpretation.* (Princeton Studies on the Near East.) Princeton: Princeton University Press. 1970. Pp. viii, 118. $5.00.

The role of the Christians of Syria in Islamic society and culture from the early Islamic period to modern times is expounded here as illustrating a sociological theory of marginals, or minorities. Despite brief remarks on the early Islamic period, the book focuses on the Ottoman period and is another offering in the old debate over the role of Christian Arabs in modern Arab history. The thrust of the presentation is that "as the Western assault demonstrated the inadequacy of traditional Islamic institutions, the Syrian Christians, because of their earlier exposure to the West and their understandably greater receptivity to its various messages, assumed once more the transmitter's role" (p. 5). The earlier exposure of the Syrian Christians to Europe, limited as it was to exchanges involving the clergy, is unlikely to have introduced into the Arab world the European concepts in question. Nor is there any need to assume greater receptivity to European ideas on the part of Christians. In point of fact the introduction of European ideas, including patriotism and nationalism (with an incipient secularism), began as the result of efforts by the Ottoman and Egyptian governments to Westernize the military and the administration. Ottoman and Egyptian state educational activities were the crucial element. To treat the Syrian Christians in isolation does violence to the historical reality of a cultural unit whose major centers were Cairo, Istanbul, and Beirut. The shakiness of the main thesis aside, this essay has solid merits. The author has delved deeply into unused sources, published and unpublished, European and Arab, ecclesiastical and secular. After clarifying the attitudes of the two major Christian communities in Syria—the Melkites and the Maronites—to the early Roman Catholic missionary activity, he shows how the radical decline in Ottoman power relative to Europe between 1650 and 1750 led to a vast increase in the number of Uniates and, above all, the creation of the Uniate Melkites. The distinctive courses in cultural and political affairs that the resultant three communities subsequently took, including their several positions at the present time, are reviewed succinctly and precisely. In all these matters Haddad has utilized his sources with skill and discernment.

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V. D. SEGRE. *Israel: A Society in Transition.* New York: Oxford University Press. 1971. Pp. viii, 227. $9.95.

V. D. Segre has produced a study of the evolution of the Jewish national movement that is imaginative, critical, and direct in its dealings with the most difficult problems facing Israel today. There is no ambiguity in the position of the author, and his partisanship is neither pretentious nor disguised. This is as much a welcome addition to such political-historical studies as is the humanistic interpretation of modernization, which the author insists that this work illustrates, with reference to Israel. Segre sees Israel as having developed from prestate days when the structures of the future state were already in existence, a characteristic that he feels distinguishes Israel from other developing countries, to the present along a continuum that he describes as moving from a "colonial, agrarian and traditional Messianic society into that of an independent, industrial and productivist State." It is as a result of this process, and particularly its later stages, that Segre studies the victory of June 1967, an event that paradoxically ensured the survival of the state while endorsing those very changes in its nature that may ultimately radically alter its character. This process has also exposed what Segre refers to as "the problem of Jewish national desynchronization, the stretching of its ideological content to cover its actions. . . ." It is under the general theme of desynchronic development that Segre studies the phenomenon

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of Zionism, a nationalist movement fighting anti-Semitism, which is transformed in our time into "a European Jewish settlement fighting Arab nationalism in the Middle East." The changing character of Zionism, a reflection of the change in orientation between the milieu of Eastern Europe and that of the Middle East; the changing nature of Jewish immigration; and the gradual development of the country from its agrarian emphasis to its present industrial needs—all of these transformations, gradual and separate as they may be, have combined under the simultaneous pressures of major power interests and the unresolved relationship with Palestinians and Arabs, which remains a central issue to produce the contemporary situation in Israel. It is little wonder that the relationship of Israel, Judaism, and the Diaspora is under revision, and it is also not surprising that the question of Jewish identity continues to be a permanent paradox in the midst of these fundamental changes. If he does not provide the answers to the questions he raises, at least Segre understands some of the important questions that must be raised, and that he as scholar and as a committed Israeli, wishes to see discussed.

IRENE L. GENDZIER
Boston University

JOHN MARLOWE. *Cromer in Egypt.* New York: Praeger Publishers. 1970. Pp. 332. $10.00.

The author views Cromer's role in Egypt as primarily an exercise in European diplomacy with its major objective the safeguarding of imperial communications and its major achievement the consummation of the Anglo-French Agreement of 1904. Whatever administrative successes Cromer achieved were incidental to his major task.

However the British may have defined Cromer's responsibilities, it is clear that there was more to Cromer's work in Egypt than Marlowe suggests. He is undoubtedly correct that the British were anxious to safeguard their imperial communications and were not interested in creating an imperial administration in Egypt, but they could have achieved their objectives without so deep and long an involvement in the internal affairs of Egypt. In a word, the narrowness of Marlowe's theoretical framework leaves too much unexplained, including the quotation from Salisbury that appears on the book's dustjacket: "If the world were falling to pieces round his ears, but Egypt was left intact, Lord Cromer would not ask for more."

Lord Cromer, perhaps to satisfy some psychological needs of his own, had a commitment to Egypt that was beyond the mere task of safeguarding Britain's lines of communication. There is something about Egypt that evokes an impulse in persons and nations to set things right. Even the Russians today have succumbed to the same impulse. Like the British before them the Russians may face bitter disappointment, but the pursuit of national self-interest alone does not explain the desire of nations to rescue Egypt from its own mistakes and its desperate social and economic problems. On both a private and a national level people frequently feel solicitous of Egyptians, and Cromer must genuinely have wished to help them. His whole career in Egypt makes sense only if we acknowledge such a commitment on Cromer's part. Whatever the final judgment on Cromer—and Marlowe's work does not provide that satisfactorily—his labors in Egypt's behalf went beyond mere diplomatic necessities.

Marlowe's work is dissatisfying in other ways also. He quotes too heavily from the sources without incisive analysis of what these sources mean. Also, his sources are too official and scarcely go beyond diplomatic dispatches and Cromer's annual reports. Although he lists private papers in his bibliography, Marlowe offers very little new evidence. Furthermore, he tells us almost nothing about the kind of person Cromer really was beyond the fact that his nickname among his colleagues was "Over-Baring," which may provide a clue to the man's personality but is really inadequate to explain his relations with such diverse personalities as the Khedives Ismail, Tawfiq, and Abbas Hilmy II; and Muhammad Abdu, Saad Zaghlul, Nubar, Riaz, and others. The financial matters that occupy so much space in the book might well have been sacrificed to other things. An economic historian should someday soon write a monograph on this subject for which much documentation exists both in Europe and Egypt. Other writers will then no longer need to deal with this tiresome and very confusing story. Finally, because of the author's narrow

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conceptual framework, he does not deal adequately with the interaction of either British or Egyptian internal politics and the policies of Cromer and Great Britain.

We have, then, in Marlowe's book another dimension from which to view Cromer's work in Egypt, and for students of the period who have no familiarity with this side of the story, Marlowe's book will prove a useful summary of known materials. This book does not, however, represent the last word on the subject of Cromer and his work in Egypt. I hope that someone will soon undertake a study that will go beyond what has already been written.

HELEN ANNE B. RIVLIN
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ASIA AND THE EAST

SHIRLEY S. GARRETT. *Social Reformers in Urban China: The Chinese Y.M.C.A., 1895–1926.* (Harvard East Asian Series 56.) Cambridge, Mass.: Harvard University Press. 1970. Pp. 221. $7.50.

Rather than heal or educate, the first Protestant missionaries in China strove to preach to the Chinese and make converts, but with meager results. They failed to reach the upper classes or gain the favor of the Chinese government. Their critics suggested that they educate Chinese, disseminate scientific information, and show greater concern for Chinese social needs. But the organization that responded with greater success and Chinese approval than any mission was the YMCA.

Mrs. Garrett uses YMCA archives, the papers of its secretaries, and interviews to put together the story of the Chinese YMCA from 1895 when David Lyon, the first official YMCA secretary, reached China until 1926 when a torrent of revolutionary criticism jeopardized the association. Its revival later under the Nationalist Government she indicates but does not include.

Beginning in England in the 1840s and in the United States in the 1860s, the association prepared itself by developing a program of service and of simplified, optimistic Christianity for city-dwellers. Later it recruited student volunteers for work overseas.

Appearing opportunely when reformers sought agencies for Sino-Western collaboration, the YMCA established branches in the treaty ports, sponsored lectures on science, taught Chinese vocational skills and reading, developed a program of athletics, and provided recreational facilities. It scheduled Bible classes and prayer meetings, but the major emphasis was on service. Students and workers were attracted, so also the favor and contributions of leading businessmen and government officials. By 1912 it had 75 Western and 85 Chinese secretaries, 25 city and 105 student associations, and almost ten thousand members. In 1922 there were 81 foreign and 378 Chinese secretaries and 54,000 members in 36 cities.

But after 1919 leaders of the May Fourth movement and student revolutionaries came to regard the YMCA as a tool of the bourgeoisie; anti-Christian critics attacked it. Vulnerable because of its business support and Christian affiliation, the association tried to be neutral and gradualist but could not adjust adequately. So it forfeited labor and student support.

Mrs. Garrett is too brief. She owes her readers an expanded volume on her topic and a sequel for the years 1926 to 1937.

EUGENE BOARDMAN
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TA-LING LEE. *Foundations of the Chinese Revolution, 1905–1912 (An Historical Record of the T'ung-meng Hui).* (Asia in the Modern World, Number 8.) [New York:] St. John's University Press under the auspices of the Center of Asian Studies. 1970. Pp. viii, 264. $3.00.

MARY BACKUS RANKIN. *Early Chinese Revolutionaries: Radical Intellectuals in Shanghai and Chekiang, 1902–1911.* (Harvard East Asian Series 50.) Cambridge, Mass.: Harvard University Press. 1971. Pp. 340. $10.50.

Until about a decade ago, the prevailing interpretation of China's 1911 Revolution largely coincided with the Kuomintang's view of its own history. In that interpretation the T'ung-meng-hui, direct ancestor of the Kuomintang, was a coherent and powerful organization that led the overthrow of a corrupt, reactionary, and alien (Manchu) dynasty; Sun Yat-sen, leader of the T'ung-meng-hui, provided the revolutionary movement with its ideology and organization and secured for it international

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sympathy and support; the overseas Chinese, especially in Southeast Asia and the United States, contributed vital assistance to Sun and to the republican revolution; Sun and his supporters were mostly modern-minded democrats, and even exceptions such as Sun's secret society allies could trace their lineage back to an honorably anti-Manchu, nationalistic, genuinely revolutionary, popular mass movement. Adherents of this view also tended to regard Sun's opponents (including advocates of constitutional monarchy such as Liang Ch'i-ch'ao and his followers) as incorrigibly backward-looking traditionalists, unswervingly loyal to Manchu despotism in general and to the Kuang-hsü emperor in particular. Finally, the 1911 Revolution, though betrayed by Yüan Shih-k'ai, was held to be enough of a political success to constitute the major turning point in modern Chinese history.

This interpretation has never gone unchallenged, but attacks upon it have intensified in the last ten or twelve years; while it may not have been demolished, it has been in peril for some time. This may account for the defensive tone of Ta-ling Lee's book, which asks us "to appreciate [T'ung-meng-hui] courage and farsightedness properly" and which questions Kuomintang historians on a few points but agrees with them in all essentials. Indeed the book can be regarded as a convenient summary of older scholarship on the 1911 Revolution. Although flawed by some serious weak spots (notably the discussion of socialism), and by more than a few errors and inconsistencies—some of which are attributable to writing, editing, and printing that leave much to be desired—Dr. Lee's description of T'ung-meng-hui history, organization, principles, and military efforts is useful and, on the whole, probably the best available in English. His interpretation, however, lacks force because it fails to address itself to the issues that scholars have raised in recent years. A rebuttal to those who have questioned Sun's leadership, T'ung-meng-hui primacy, Manchu obscurantism, Liang's conservatism, and the importance of 1911–12 as a turning point would be stimulating and most welcome; but in this book the old chestnuts are served up much as they were harvested many years ago, and they taste correspondingly stale.

Mary Backus Rankin's book exemplifies the new scholarship. Dr. Rankin calls our attention away from Sun, the T'ung-meng-hui, and revolutionary bases outside China and directs us to lesser known revolutionary leaders and organizations in Shanghai, China's most Westernized city, and in the less urban areas of nearby Chekiang province. She also asks us not to fasten exclusively upon the political changes of 1911–12 but to assign more weight to the new ideas, values, and social and political relationships that appeared in the entire decade preceding the fall of the Manchu dynasty, especially the period, 1902–07, when "student radicalism made its greatest contribution to the revolutionary movement." But that contribution, she finds, was less to the 1911 Revolution than to an upheaval that has continued throughout the twentieth century.

What, then, are we to make of the 1911 Revolution? Dr. Rankin speaks powerfully to this question, for although she emphasizes the intellectuals' work in 1902–07 and its relationship to post-1911 radicalism, she has also gathered an immense wealth of other data. Firmly in control of a wide range of sources, Dr. Rankin has given us a detailed social history of a limited area during China's first revolutionary decade, and she has managed to illuminate both the similarities and differences between urban and rural movements of the time; in these respects the book is a major landmark in modern China studies. The T'ung-meng-hui is shown to have played a greater role in Shanghai and the larger cities of Chekiang than in smaller cities and towns, but everywhere there was a similar pattern: The revolutionary movement became much bigger than the T'ung-meng-hui as it spread beyond a handful of intellectuals and secret society members to include soldiers, police, merchants, gentry, and members of the new, Manchu-sponsored provincial assemblies; and, as it became the work of a broad and loose coalition, the revolution became less revolutionary. The radicals radicalized the moderates, but the latter also moderated the revolution. A fragmented, highly diverse movement—subject to no continuing leadership and to no discipline or organization, united only by a mood and by the most flimsy and superficial agreement on a few general issues—gave birth to a deformed republic. And in this account

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the traditional father of the Chinese Revolution, Sun Yat-sen, and the villain who betrayed him, Yüan Shih-k'ai, are less in evidence than Ch'iu Chin T'ao Ch'eng-chang, while the influence of Liang Ch'i-ch'ao and the constitutionalists—or "royalists," in Dr. Lee's term—is everywhere.

It may still be possible to agree with Dr. Lee that without the T'ung-meng-hui there would have been no republican revolution in China, but the case will have to be made on grounds other than those he has provided. With the addition of Dr. Rankin's book to the work of the last decade, no other point in the Kuomintang interpretation of 1911 seems likely to survive.

MICHAEL GASSTER
Livingston College, Rutgers University

PAUL K. T. SIH, edited with an introduction by. *The Strenuous Decade: China's Nation-Building Efforts, 1927–1937.* (A symposium in celebration of the St. John's University Centennial, 1870–1970. Asia in the Modern World Series, Number 9.) [New York:] St. John's University Press under the auspices of the Center of Asian Studies, 1970. Pp. xxi, 385. $10.00.

One of the dreariest debates about modern China is whether Chiang Kai-shek was doing a good job of "nation-building" before he was rudely interrupted by Japan in 1937. An affirmative answer is presumed proof that Kuomintang (KMT) leadership was an adequate vehicle for "modernizing" China and that the Communist victory was the result of historical accident or of American treachery. By the time we get through the layers of faulty assumptions underlying the question, the answer becomes a matter of secondary importance. The concept of "nation-building" too easily loses analytic utility and degenerates into a convenient device for coming to terms with the "third world" within a nonrevolutionary frame of reference.

This volume, the product of a St. John's (New York) conference in July 1969, illustrates the deficiency of the nation-building question and the irrelevance of the answers. The nineteen contributors are long on experience and know-how, but short on perspective. The four Americans among them are men with decades of diplomatic, advisory, and academic association with the KMT. Most of the fifteen Chinese are former Nationalist officials now teaching and writing in this country. Nearly all of them hold advanced degrees from American universities, which may help to explain their tacit assumption that China's problems were solvable through technical reforms under the direction of highly trained experts. Nine chapters cover diplomatic relations, political reconstruction, fiscal reform, currency and banking, agriculture (two chapters), industry, railroads, and education. These constitute the maximum case yet made for KMT achievements during its "golden decade" and should serve as a benchmark for those embarking upon more critical evaluation of the record. The contributors "unanimously reached one and the same conclusion," states editor Sih: "During the decade 1927–1937, the Chinese National Government, with the Kuomintang as the ruling party, did a superb task in nation-building development. In the midst of 'internal worries' and 'external threats,' the Government was able to carry out the national program with efficiency and effectiveness. Policy was sound; leadership was intelligent. Had it not been for the outbreak of the Resistance War against the Japanese invasion in July 1937, China would have been able to attain the stature of a new, modern society, whereby the Principles of the Three Peoples [*sic*] would have been fully realized.

Although "internal worries" and "external threats" echo like choral motifs in chapter conclusions, few authors are as uncritical as the editor. The careful reader will find the KMT chided for a counter-productive and regressive system of taxation, failure to create sound rural credit institutions, inability to enforce its own rent reduction laws, the stifling effects of centralized educational control, and failure to improve the economic life of the people.

However, neither authors nor commentators give serious consideration to the alternative hypothesis that endemic shortcomings rendered the KMT regime exceptionally vulnerable to the crises to which it succumbed. There is no analysis of factionalism within the ruling elite or conflict between socioeconomic groups in Chinese society. KMT failures in the rural sector are blamed on shortcomings in specific programs rather than on more fundamental disorders. The Chinese farmer, writes one author,

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was a "good, honest, simple, and peace-loving" soul, thoroughly apolitical. Strangely, there is not even a chapter on military modernization, an area in which Chiang achieved striking, though short-run, success.

One reason for the KMT's collapse was that its skilled, modern experts were out of touch with the revolutionary potentialities of their own society. For Americans who still seek to "build nations" in cooperation with "our kind" of English-speaking urbanites, the Kuomintang's golden decade is well worth further study.

JOHN ISRAEL
University of Virginia

J. D. SIMMONDS. *China's World: The Foreign Policy of a Developing State*. New York: Columbia University Press. 1970. Pp. ix, 260. $10.00.

PETER VAN NESS. *Revolution and Chinese Foreign Policy: Peking's Support for Wars of National Liberation*. (The Center for Chinese Studies at the University of California, Berkeley.) Berkeley and Los Angeles: University of California Press. 1970. Pp. xii, 266. $6.50.

CHAE-JIN LEE. *Communist China's Policy toward Laos: A Case Study, 1954–67*. (International Studies, East Asian Series. Research Publication, Number 6.) [Lawrence:] Center for East Asian Studies, University of Kansas; distrib. by Paragon Book Gallery, New York. 1970. Pp. xii, 161. $5.50.

These three volumes are timely studies on a subject of increasing importance now that the People's Republic of China is expected once again to play an active role in international affairs. Besides sharing a common conclusion that Peking's foreign policy has not always been circumscribed by ideological considerations, the three studies are vastly different in scope, methodology, and degree of scholarly excellence.

The Simmonds volume is the most disappointing. The author, who is a member of the Australian Defense Department and who has lived in China for brief periods, claims in the introduction that his is a new method of interpreting foreign policy statements "based on the analyses of the overall domestic developmental pattern." Whatever this means, more than half of the book, in fact, is devoted to what he calls "chronology of institutional change," which is nothing more than a month by month summary account of events in China punctuated by frequently obfuscating commentaries. The only original portion of this chronology in three inordinately long chapters is the listing of periodic changes of vice-ministers of foreign affairs and their probable responsibilities, although the significance or relevance of these changes is nowhere made clear. The second half of the book, in eleven short chapters, purports to be an analysis of Chinese foreign policy themes, but again it is a superficial explanation of such recurrent terms as "paper tiger" and "the East Wind." It adds nothing to the already available secondary material that the author does not appear to have consulted. It has no bibliography. In my opinion the book should never have been published.

In contrast the work of Van Ness is more limited in scope, competent in design, and innovative in methodology. It is a study of China's relationship with revolutionary movements in Asia, Africa, and Latin America. By using a simple, but evidently well thought out, schematic analysis of the Chinese press, the author selects a group of countries where there were revolutionary movements supported by the Chinese in 1965. He finds that some of these countries, which he calls "targets for revolution," are neither colonial nor feudal, and therefore Chinese support for these revolutionary movements was not based on Communist ideology. Furthermore, on the basis of a friendliness-hostility criterion he arrives at the conclusion that Chinese support of foreign revolutionary movements is determined primarily by the position of a particular government vis-à-vis Peking, and secondarily by such other factors as geographical proximity to China, degree of United States involvement, and revolutionary potential.

One may argue that his selection of a single year for the study is too narrow to be representative and that the methodology could have been refined to make it possible to determine the degree of Chinese support through thematic analyses. Nevertheless the Van Ness thesis is a viable one, and it is hoped that it will stimulate further empirical studies to test its validity.

Chae-Jin Lee's superbly written monograph on China's policy toward Laos, 1954–67, generally confirms Van Ness's findings. By examining currently available information Lee finds that Chinese policy, despite the alternation be-

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tween militancy and moderation, is rational and prudent. Above all, the author suggests, "the Chinese attached the highest priority to their national security interest. . . . This security concern made the Chinese sober and cautious in using their military power as a means of their foreign policy in Laos," especially in view of the deepening involvement of the United States after the second Geneva conference. Other determinants of Chinese policy, the author believes, are the developments within Laos such as the ascendancy of Phoui Sananikone in 1959, the status of the Pathet Lao, and the posture of the Soviet Union.

Lee's book is well organized in five moderate-length chapters and has an excellent bibliography. It is as informative as Van Ness's is thoughtful.

S. M. CHIU
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SHUMPEI OKAMOTO. *The Japanese Oligarchy and the Russo-Japanese War.* (Studies of the East Asian Institute, Columbia University.) New York: Columbia University Press. 1970. Pp. x, 358. $12.00.

Professor Okamoto has produced a valuable study of the decision-making process in the Japanese government at the time of the Russo-Japanese War. This is the first in-depth study of the interaction of domestic politics and war and diplomacy during one of the most crucial periods of Japan's history, a period in which her very existence was at stake. The author has made extensive use of materials heretofore neglected or inadequately utilized by scholars.

At the time of the war decision making was vested in a group of fourteen men made up of the emperor, the five elder statesmen, five cabinet ministers, and three top military leaders. As Okamoto points out, this "oligarchic leadership demonstrated a high level of competence and served the state well." These men of experience and judgment as well as foresight were not swayed by the clamor of public opinion in their decision to go to war or to make necessary preparations for an early peace settlement. By never losing sight of the principal war aims and by carrying out maximum coordination between military strategy and diplomatic moves, they brought about an opportune cessation of hostilities and a realistic peace settlement, thereby enabling Japan to achieve international recognition and a new status in terms of world power.

The author points out that "the changing of the guard" that occurred in 1901 with the formation of the Katsura government—which did not include a single elder statesman but was made up entirely of second-generation leaders—eroded the power of the Genro, who had to defer to the younger hard-line policy advocates. The latter included Premier Taro Katsura and Foreign Minister Jutaro Komura, who anticipated war with Russia and made necessary diplomatic and military preparations. The leaders were farsighted, for at the time of the commencement of hostilities they were already thinking seriously of how to end the war. Komura had a concrete plan for peace as early as July 1904, and Premier Katsura by August. Field Marshal Iwao Oyama, commander-in-chief of the Manchurian army, knew when to let diplomacy take over to bring about peace.

This is a work of major importance. It will be especially useful to students of international relations who are interested in the interaction of domestic politics, war, and diplomacy and in the decision-making process as it operated in Japan at the beginning of the twentieth century.

CHITOSHI YANAGA
Yale University

BEN-AMI SHILLONY *et al. Crisis Politics in Prewar Japan: Institutional and Ideological Problems of the 1930s.* Editor: GEORGE M. WILSON. (*Monumenta Nipponica* Monograph.) Tokyo: Sophia University. 1970. Pp. xiii, 87. $3.25.

The studies in this thin volume were originally presented in shorter form as a panel at the annual meeting of the Association for Asian Studies in 1969. The contributors seek to illuminate the inner dynamics of specific domestic crises and pressure groups in Japan during the 1930s without the encumbrance of the "drift to the Pacific War syndrome" that has so prominently characterized our historiography of Japan in the prewar decade. According to the editor's reading of Shōwa politics, such internal strife as is discussed in this monograph rather than any foreign difficulties signaled to an apprehensive civilian and military power elite an imminent radical change and impending disaster for

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Japan. To counter this tendency, to achieve its own goals of a self-sufficient economy and an end to the "China incident," and to guarantee Japan's autonomy this elite suppressed social and political agitation and fostered a conservative reaffirmation of Meiji values that placed a high priority on law, order, and stability.

The three substantive articles in the monograph are ably researched and attest to the growing sophistication of American scholarship on modern Japanese history. Richard Smethurst writes of the virulent attacks by chauvinistic, xenophobic local leaders of the Military Reserve Association on Professor Minobe Tatsukichi's "organ theory" of the state. While most military men might share such ethnonationalistic views, the central authorities of the association and both active services feared a disciplinary breakdown. This was averted, and political activity stopped as the army and navy took over direct control of the association.

The fascinating study of the February 26 affair, by Ben-Ami Shillony, is alone worth the price of the book. Shillony points up the limitations of the Kōdō-Tōsei interpretation as he broadens our perspective beyond those military men involved. He emphasizes that this most serious attempt at a coup d'état in twentieth-century Japan very nearly succeeded because of the extent of support by powerful elements of both the civil and military establishments. I was intrigued by his disclosure of those supporters close to the emperor and by the emperor's explicit opposition to the "rebels."

Robert Spaulding's incisive "Japan's New Bureaucrats," a study of like-minded, pragmatic nationalists termed "revisionists," represents careful analysis and intellectual discrimination. Spaulding arraigns contemporary evaluations of these revisionists as Communist revolutionaries as well as those of postwar Japanese writers who label them as mere fascists. He concludes that the prewar army-revisionist partnership was not as unequal as has been suggested.

Although the editor appears to have credited this work with more than should be reasonably expected, the excellence of these studies bodes well for these young scholars and for prospects for our understanding of prewar Japanese politics.

G. RALPH FALCONERI
University of Oregon

JOHN TOLAND. *The Rising Sun: The Decline and Fall of the Japanese Empire, 1936–1945*. New York: Random House. 1970. Pp. xxxv, 954. $12.95.

This book, based on extensive research and innumerable interviews, is an absorbing, fascinating, and objective account of the Pacific phase of World War II. After a brief and oversimplistic treatment of events from the army uprising in Tokyo in February 1936 to the futile American-Japanese negotiations in the summer of 1941, the author concentrates on descriptions of the planning and prosecution of the various campaigns and battles. The devastatingly successful attack on Pearl Harbor, the valiant defense of the Philippines and the excruciating and cruel march of the prisoners of war there, and the fall of Singapore and the Netherlands Indies are treated as prelude to the plans of the Japanese high command, especially of Admiral Isoroku Yamamoto, the imperial fleet commander, for a decisive battle in the Pacific. The author then shows how the navy leaders, intoxicated by these unexpectedly easy victories, hoped to isolate Australia in the south and eliminate America's naval threat by the capture of Midway, and how these ambitious plans were thwarted by the naval battles of the Coral Sea and Midway and by the six months' struggle of the Americans to drive the Japanese out of Guadalcanal in the Solomons.

The last half of the book is devoted largely to American attacks at selected points across the central Pacific, the capture of Saipan in 1944, the bombings of Japan's home islands, the sea and land campaigns for the recapture of the Philippines, the disintegration of Japan's air and sea power, the isolation of Japanese forces on the continent, the holocausts resulting from Japan's futile suicidal tactics in defense of Iwo Jima and Okinawa, the search for peace, the effect of the atomic bombs on Hiroshima and Nagasaki, and reaction of the nation to capitulation.

The method of presentation of some of the material and the heavy reliance on personal interviews as sources raise certain questions for the historian. In his preface the author states that dialogues, which are numerous, are not fictional but come "from transcripts, records and stenographic notes and the memories of the participants." But there are no reference foot-

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notes throughout the book, only general bibliographical notes for each chapter. How reliable is anyone's memory after twenty-five years? How many of the printed memoirs or International Military Tribunal documents used as sources are based on faulty memory or on only fragmentary written records? If the material in this book is to be used as basic source material, the historian needs either exact citations or more specific reference information.

Finally, there are certain important omissions and doubtful assumptions made by the author. In describing the Cairo Conference of 1943 of Churchill, Roosevelt, and Chiang Kai-shek, the author makes no mention of the important political declaration that set forth the territorial limitations of postwar Japan. Prime Minister Tojo's desperate efforts to unite the home front by forming the politically oriented Imperial Rule Assistance Association, the munitions ministry, and the Greater East Asia Ministry are also omitted. Furthermore, there is the assumption that key figures in the narrative were responsible for decisions and statements that actually were prepared by their subordinates. For example, the preparation of the text for the Potsdam Declaration of 1945 was the product of earlier efforts of the State-War-Navy Coordinating Committee. But this book, which will appeal to a wide range of readers, will give them a sense of the irrational decisions leading to war and the realities and horror of modern warfare.

HUGH BORTON
Columbia University

MARTIN E. WEINSTEIN. *Japan's Postwar Defense Policy, 1947–1968*. (Studies of the East Asian Institute, Columbia University. New York: Columbia University Press. 1971. Pp. x, 158. $7.50.

Professor Weinstein (Political Science, University of Illinois, Champaign) has written a crisp, lucid, and well-organized monograph, originally his Ph.D. dissertation. He begins his study with those terrible years immediately following the Pacific War when Japan held very few cards indeed in international politics. But Weinstein's thesis is that Japanese postwar defense policy has been an active and creative process, not simply a derivative one generated in response to American proddings or to the pressures of domestic pacifism. Japan has not been supine; nor has the conservative party pushed Japanese rearmament. Weinstein argues persuasively that the Japanese government since 1947 has endeavored to pursue a consistent policy that has led to the building of sufficient military strength to preserve internal security, whereas Japan has relied upon agreements with the United States to provide a deterrent against external threat, i.e. possible Russian attack. Thus Japan's defense force is appropriate for a strategic policy deemed by the Japanese government suitable for Japan. Unfortunately, as Weinstein says, the Japanese people have not appreciated the skill of the diplomacy behind this policy; nevertheless the Conservative government has not been voted out of office since 1947.

Weinstein ends his study in 1968, which seems a bit arbitrary. In the light of more recent events it might have been more appropriate to carry the story up into the new decade, at least until June 1970, the expiration of the first phase of the Security Treaty of 1960. The book is a good one; it fills a significant gap in the none too abundant literature concerning postwar Japan.

J. C. PERRY
Carleton College

JAMES W. WHITE. *The Sōkagakkai and Mass Society*. (Stanford Studies in Comparative Politics, Number 4.) Stanford: Stanford University Press. 1970. Pp. xii, 376. $12.95.

This is an excellent book on the Sōkagakkai. The Sōkagakkai (a nationalistic religious organization whose ideas are traced to a Japanized form of Buddhism of the thirteenth century) has been one of the more visible of political phenomena in Japan in recent years and has received considerable attention in the West. Most writings on the subject, however, have been journalistic and, on the whole, quite superficial. Professor James White's book is an exception in this regard. It is a serious monograph, excellently written, organized, and argued. The book is also provocative. Unlike most characterizations it evaluates the organization and activities of the Sōkagakkai in sympathetic and positive language.

The book is divided into two large parts. The first provides an internal analysis of the Sōkagakkai. It is by far the best concise treat-

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ment in Western language of the organization, ideology, social content, and overall internal dynamics of the Sōkagakkai movement.

Of greater theoretical and interpretive significance is the second part of the book, which attempts to relate the Sōkagakkai to definitions of "mass society" and of political socialization as postulated by William Kornhauser, Seymour M. Lipsett, Sidney Verba, and others. This is a fascinating approach, the likes of which are very scarce in political studies on Japan. In general, White argues that (contrary to certain prevailing assumptions) the Sōkagakkai does not fit the definitions of alienated "mass man" in "mass society." This argument is based on evaluations of "alienation," "authoritarian," and "acquiescence" indices. White argues that members of the Sōkagakkai are not as intolerant, authoritarian, and exclusivist as is generally held. Certainly they are not sharply distinguishable from the general norm in the social environment as a whole.

White stresses the point that the Sōkagakkai is much like the mainstream of political society, adaptive and committed to procedures of parliamentary politics. In surprisingly unequivocal language he writes that there is "sincere formal adherence to democratic values and processes" and "strong and formal commitment to secular parliamentary democracy." In attempting to show some of the incongruities between the Sōkagakkai and the Western model of "mass society," White may have overstated his case. The Sōkagakkai may want at this point to project a democratic image. But tactic is one thing, sincere commitment is another matter.

As with other books that treat political events in process, White's analysis will no doubt be revised, modified, or perhaps shown to be incorrect as the Sōkagakkai develops. It is, however, an important book. It establishes a substantial and provocative point of reference for all subsequent work on the Sōkagakkai and of mass movements in postwar Japan.

TETSUO NAJITA
University of Chicago

AMERICAS

JOHN E. POMFRET, with FLOYD M. SHUMWAY. *Founding the American Colonies, 1583–1660.* (The New American Nation Series.) New York: Harper and Row. 1970. Pp. xvii, 380. $7.95.

This volume in the New American Nation series is completely characteristic of its author; it is a meticulous, judicious, balanced, and very modestly stated piece of work. Few historians have contributed more to American colonial scholarship than Dr. Pomfret, with his books and articles on early New Jersey, his sponsorship of the Institute of Early American History and Culture, and his administration of the Huntington Library—and never was there a less ostentatious craftsman. Pomfret's present book is a clear, concise survey of all English colonizing efforts in America from Humphrey Gilbert's landing at Newfoundland to the restoration of Charles II. The emphasis throughout is political and institutional. Each colony is given a separate chapter (Virginia and Massachusetts get two apiece), with prime attention paid to colonizing methods and governmental structure. The narrative is fact filled, with little room for romance or passion. Puritanism, for example, is never seen as an all-consuming force in early New England, and once the saints settle into the business of making a living in the wilderness they quickly lose their religious dynamism. Pomfret's approach is very much in the vein of Charles M. Andrews, and, appropriately, he cites Andrews's *Colonial Period of American History* (1934–38) far more often than any other authority. But he also takes full account of more current scholarship, drawing in several cases upon books published only a few months before his own.

Pomfret's colony-by-colony presentation enables him to allot more space than is customary to the northern outposts in Newfoundland, Nova Scotia, and Maine, and to the island colonies of Bermuda, Barbados, the Leewards, and Jamaica. These chapters are especially welcome. The disadvantage, of course, with Pomfret's organizational arrangement is that he cannot give a general view of social, economic, and psychological experiences common to all the pioneer settlements. For instance there is no overall assessment of race relations in this crucial formative period, though one may infer from the author's comments on the Pequot War and on Negro servitude in Virginia that the early colonists treated the Indians and blacks pretty well. On this topic as on others some readers may wish that Dr. Pomfret had taken a more definite stand and that he had pointed out the issues that most need further

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study. Instead we have an expert synthesis of present knowledge, an evenhanded narrative, buttressed by an extensive and highly useful bibliography, which will serve a wide audience for many years to come.

RICHARD S. DUNN
University of Pennsylvania

MICHAEL AARON ROCKLAND, translation and introductory essay by. *Sarmiento's* Travels in the United States in 1847. Princeton: Princeton University Press. 1970. Pp. xiii, 330. $9.50.

Sarmiento's *Travels in the United States in 1847* is the first complete English translation of the third section of his *Viajes en Europa, Africa i America*, which appeared in 1852. In 1845 the Argentine educator and journalist had published *Civilización i barbarie*, a masterly indictment of *caudillismo* and the government of Juan Manuel de Rosas. Although brought out in Santiago where Sarmiento was in exile the work was almost immediately known in Buenos Aires, and the *porteño* dictator asked the Chilean government to extradite the author on the grounds of treason. The Chileans had no intention of handing over a man they had appointed to teach at the university, especially as he had recently founded their first normal school, but, in order to ease tension between the two countries, the minister of education, Manuel Montt, invited Sarmiento to visit Europe and the United States and make a survey of educational systems abroad. He knew that the thirty-four-year-old apostle of school development from across the Andes would bring back extremely useful information, and he was not disappointed.

Few nineteenth-century travelers got the point of the United States more quickly than Sarmiento, and he set down his impressions with perception, wit, and understanding. As a South American he had expected to find the basic cultural and educational values he was seeking in the Old World, but he had been disappointed: "In the European monarchies, decrepitude, revolution, poverty, ignorance, barbarism, and the degradation of the greatest number are always found together." When he landed in the United States he saw a country bustling with energy, towns and cities and factories springing up, and thousands of people moving westward. Railroads had been built, boats plied the rivers and canals, and the flamboyant hotels took his breath away: "Every large city in the United States boasts of two or three monstrous hotels that compete among themselves in offering luxury and comfort to the public at the lowest prices. . . . Hotels have an extremely important role in the domestic life of nations. . . . In dynamic societies, with an active life and a future, the hotel will be more important than any other kind of public construction." He liked the pragmatic approach to life: "I have come to the conclusion that if you want to know if a machine, an invention, or a social doctrine is useful and can be applied or developed in the future, you must test it on the touchstone of Yankee knowhow."

His greatest admiration, however, was for Horace Mann, whose school system was being developed in Massachusetts and with whom he has several long conversations. "Can you think of anything more beautiful than Mr. Mann's obligation as Secretary of the Board of Education to travel a part of the year, call an education meeting of the population of every village and city where he arrives, mount the platform and preach a sermon on primary education (demonstrating the practical advantages which accompany its wide diffusion), stimulate the parents, conquer selfishness, smooth out difficulties, counsel the teachers, make suggestions, and propose improvements in the schools which his science, his good will, and his experience suggest to him?"

Sarmiento predicted that the United States would become a great nation, though he saw its flaws. He pointed to the dishonesty of politicians in the state governments, and he was astonished by the lack of manners among the masses. Still, he condoned this: "Europeans make fun of these rude habits, which are more superficial than profound, and the Americans for the sake of argument, become obstinate and justify them as going hand in hand with liberty and the American way of life. I do not mean to defend or excuse these characteristics. Still, after examining the chief nations of Christendom, I have come to the conclusion that the Americans are the only really cultured people that exist on this earth and the last word in modern civilization."

Professor Rockland's long, excellently written, and illuminating preface is both a condensed biography of Sarmiento and a guide to the *Travels*. It describes the political, eco-

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nomic, and intellectual conditions of the period and greatly clarifies the observations of the author. There are occasional explanatory notes to the text, and one only wishes there were more. This is a book that should be in the library of all serious students of the hemisphere, for it is of equal interest to people above and below the Rio Bravo.

CARLETON SPRAGUE SMITH
The Spanish Institute

GEORGE R. STEWART. *American Place-Names: A Concise and Selective Dictionary for the Continental United States of America.* New York: Oxford University Press. 1970. Pp. xl, 550. $12.50.

Students of American place-names will find George R. Stewart's latest contribution to etymological research a welcomed reference material. The author has drawn on a wide spectrum of the literature of American place-names contributed over the past 150 years to succinctly describe some twelve thousand toponyms in dictionary format. Toponyms included were selected according to a system outlined in the dictionary's preface. Although several of the criteria for exclusion could be debated (the author's total elimination of place-names derived from personal names, the dropping of names perceived as "obsolete," and the omission of "minor" places from consideration), his restricted focus on names of "well-known" places, repeated toponyms, and unusual names that "arouse controversy" seems reasonable. The evaluation of past toponymic inventory, also located in the author's preface, identifies those areas of the continental United States for which place-names are both adequately and inadequately researched; the states of Arkansas, Georgia, Indiana, Mississippi, Tennessee, and Virginia fall into the latter category. As the author draws from state inventory materials for much of his information, these states are seen as areas for which the dictionary holds reduced reliability. For scholars interested in the cultural evolution of the Ohio and Mississippi Valleys, such a deficiency, relating as it does to the important cultural hearth of the "Upper South," poses a particular challenge for future toponymic research.

The dictionary's introduction and bibliographic section offer additional literature review focused on the etymological approaches to place-name study. Here linguistic transfer, dual and multiple origins, folk names, evolved and bestowed names, and naming systems are discussed along with additional topics to give the reader a general background in place-name analysis. A classification of place-names according to their mechanism of origin (that is, descriptive, associative, possessive, incident-derived, commemorative, commendatory, coined, transferred, folk, and "mistaken" names) is offered. The dictionary's entry system is also elaborated clearly, establishing the rationales for descriptive emphasis. Most entries attempt to give "the reason for the application of the term, the date of naming, the namer, and the occasion." In numerous entries "some specific examples may be given of typical or exceptional or of especially interesting or important occurrences." Again, one could take issue on several of the rules chosen by the author to describe and order his material. For example, the identification of American Indian place-designations by historic tribal names and not according to linguistic stock (or both) may not, as the author admits, satisfy many specialists. By and large, however, the author has chosen his procedural rules carefully. The result is a masterful addition to the literature of American place-names: a well-organized and highly readable dictionary for ready place-name reference.

JOHN A. JAKLE
University of Illinois, Urbana-Champaign

JOHN FRANCIS BANNON. *The Spanish Borderlands Frontier, 1513–1821.* (Histories of the American Frontier.) New York: Holt, Rinehart and Winston. 1970. Pp. x, 308. Cloth $6.95, paper $4.50.

Father Bannon, one of Bolton's most prominent and prolific disciples, has, on the eve of his retirement from St. Louis University, published a distillation of his long study of the Spanish Borderlands. This work appears as a volume of the Histories of the American Frontier series under the general editorship of Ray Allen Billington. It is quite probable that this work will enjoy the greatest sale of any, not because of its being any better than the others, but rather because it fills a long-standing need

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for a textbook on the Spanish Southwest. For half a century, since the appearance in 1921 of Bolton's classic *The Spanish Borderlands* in the Chronicles of America series, the southern fringe of the United States has never been without its champions, but surprisingly it has never before had a synthesized presentation for the Spanish period alone. Several writers have treated the geographical area for a more extended period, others have covered the Spanish period in briefer geographical scope, while still others have dealt with both a lesser area and a greater time span.

Written as it is by a Jesuit historian, the work possibly includes too much treatment of the religious phase, but this is where the book is at its strongest. In a synthesized account the best the author can hope for is that his book escapes criticism in all portions save that of the specialty of his reviewer. In 238 pages of text, Bannon can only skim the surface of the Borderlands' rich history, yet in the copious bibliography and footnotes it is evident that the skimming is based on a balanced study of available sources. Whereas Bolton's pioneer work was a series of connected essays on discovery and exploration, Bannon's book is an overall view of the area extending from Florida on the East Coast to the Californias on the West Coast. It is written in the straightforward manner of the historian who has much to say and little room to say it in, with no great effort to be either pedantic or cute. The Borderlands story is recounted as the great pageant that it was: a story of explorers, Indians, missionaries, soldiers and settlers; a tale of developments and institutions, of international rivalry and national inadequacy, of organization, reorganization, and disorganization. It is the story of the indelible impression left by Spain that Father Bannon brings to life in a single brief volume.

DONALD C. CUTTER
University of New Mexico

MARGARET G. MYERS. *A Financial History of the United States*. New York: Columbia University Press, 1970. Pp. viii, 451. $11.95.

There are not so many financial histories of the United States that another should not be welcomed. More to the point is the warmth of the welcome to be extended. To condense so vast an amount of material covering almost 350 years of financial history into 410 pages is indeed a formidable task, and Professor Myers has performed admirably within this obvious limitation.

Starting almost with the first settlements, the book proceeds chronologically up to the present time in an orderly, interesting, and well-written fashion. About one third of the book deals with the period prior to the Civil War; another third carries the story from the Civil War to the beginning of the New Deal; and the final third is devoted to the New Deal, World War II, and the postwar period.

Since financial history is part of a broader spectrum of economic and political life, the book is also involved with various aspects of economic history and political commentary that tend to make the treatment more thorough and more understandable. On the other hand, the attempt to condense so much material into a relatively small book is not without its consequences. While the expediencies that the nineteenth century used to solve the problems of coinage, currency, and central banking are all covered, in certain cases a more expanded treatment could be desired. This is especially so in the treatment of Biddle's last few years as president of the Second Bank of the United States and in the treatment of his egocentricities.

What is lacking in detail in the early period, however, is more than made up by the extent of the coverage of what might be called the modern period. Here the confusion of the money managers and their inability to cope with war and postwar problems is brought out with clarity and insight. The combination of pragmatism and political expediency runs through twentieth-century financial management as it did through the nineteenth. With all the advances made in monetary knowledge, very little seems to have brushed off onto the policy makers.

On the whole, the material is treated in a narrative rather than an analytical manner. There is little here for the econometrician and the model builder, a situation devoutly to be wished. The story is told in nontechnical, understandable language that should appeal to the general reader as well as the professional historian. A more than adequate bibliography

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is included. It is a job well done and deserving of its author.

CHARLES GILBERT
Hofstra University

JOHN GARRETT. *Roger Williams: Witness beyond Christendom 1603–1683*. [New York:] Macmillan Company. 1970. Pp. x, 306. $7.50.

Nearly two decades ago Perry Miller rescued Roger Williams from an earlier generation of scholars who depicted him as the harbinger of Jacksonian Democracy. Since that time other historians have stressed the importance of typology, a common form of seventeenth-century Biblical interpretation, on Williams's thought and have thereby enriched our understanding of his ideas about church and state. Indeed, the analysis of Williams's life and writings has reached a degree of sophistication unequaled by that of any other figure in early America. Unfortunately, Garrett's biography of Williams neither synthesized this growing literature nor added to it in any significant way. In fact he chose to ignore the complex question of typology altogether.

Garrett's goal was to rework well-known facts about Williams's career into a fresh appreciation of the man's place in American history. The results of this endeavor are not convincing. The Williams Garrett portrays, we discover, left "the security of a lawyer-gentry-merchant establishment" (p. 58) in the Massachusetts Bay Colony to embrace the despised of the earth. He choose to labor with his hands while Thomas Hooker, John Cotton, John Eliot, Hugh Peter, and a host of other New England divines cast their lot with the "upper classes." No doubt the young Williams felt uneasy mixing socially with the Barrington family, but his discomfort did not inhibit him from suing for the hand of Jane Whalley, Lady Barrington's niece. His rebuff in this love affair and his failure to share in the acclaim that Cotton and Peter received in London during the 1620s and 1630s may well have embittered the ambitious Williams. There is no persuasive evidence, however, that he decided to become the special friend of the "small farmers, traders, herders, tinkers, tailors, brewers, and printers" (p. 108) or that his jealousy of the more prominent Puritan preachers stimulated his interest in Separatism.

Much of Garrett's book is smoothly written, but despite its stylistic grace, most scholars will continue to rely on the works of Perry Miller, Ola Winslow, and Edmund Morgan.

T. H. BREEN
Northwestern University

LAWRENCE A. CREMIN. *American Education: The Colonial Experience, 1607–1783*. New York: Harper and Row. 1970. Pp. xiv, 688. $15.00.

Professors of education have provided us with a number of histories of education in the United States. Generally speaking, these books have been written in terms of the philosophy of education prevalent at the time of composition. In 1970 a large book appeared—the first of three to be devoted to the history of American education—and this book reveals no shadow of a dedication to any particular educational philosophy; rather it seeks to find out "what agencies, formal and informal, have shaped American thought, character, and sensibility over the years and what have been the relationships between these agencies and the society that has sustained them."

What Professor Cremin has written strikes the reader at first as a rather generalized work of American social and intellectual history. As one proceeds through the book, however, he begins to realize that all the facts of social and intellectual history have a bearing on the development of American attitudes, skills, and sensibilities, which are in truth education. The author's footnotes, which are kept to a minimum (mostly occurring at the ends of paragraphs), and his bibliographical essay reveal that he is a knowledgeable historian who is aware of nearly everything that has been written in American social and intellectual history.

Cremin starts his book off with six pages on the writings of Richard Hakluyt, who has not hitherto been considered an educator. The rest of chapter 1 and all of the two chapters that follow deal with the ideas of Erasmus, Sir Thomas More, William Perkins, Richard Baxter, Sir Thomas Elyot, Henry Peacham, John Colet, Francis Bacon, Michel de Montaigne, Petrus Ramus, and other European writers of the sixteenth century. Some colonists, it is pointed out, read these men, but it is clear that their books are summarized by Mr. Cremin in order to put the reader into a frame of mind

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suitable for approaching the beginning of colonial education.

When Mr. Cremin gets in chapter 4 to the actual founding of schools in America, he starts off with a discussion of the family. The family, he says, was the principal agency through which the colonist worked out his response to the new conditions of the American environment, consolidated what he learned, and transmitted that learning to subsequent generations. This chapter is followed by chapters devoted to the church, the school, the college, and the community. In England the Anglican Church was given legal power to act as an educational institution; but in the colonies there were more informal churches, and they taught, as Cremin says, "in many voices." In all colonies, however, schools were a matter of "public concernment." Mr. Cremin completes part 2 of his first book by describing the importance of education in the typical communities of the British New World in the seventeenth century, and he concludes that education was most effective in New England. His second book, covering the period from 1689 to 1783, is quite naturally longer than his first since it deals with more colonies and with many more schools.

It is a relief to have a book that deals in some detail with the schools and colleges of the colonial period to set against the background provided by Perry Miller, Samuel Eliot Morison, Bernard Bailyn, Clinton Rossiter, Edmund S. Morgan, and many other colonial historians. One has the sense here that one is reading about colonial education as a part of this complete social environment, and not, as has too often been the case, about schools nearly in isolation from the contemporary scene. This consideration of schools within their social matrix has led Mr. Cremin to make some generalizations that his predecessors in the field have not made—for instance, that the almanacs, turned out in such numbers by colonial printers, served an educational purpose. Similarly, his familiarity with general social history leads him to treat the work of the Society for the Propagation of the Gospel as primarily educational, even its work among blacks and Indians, whom, by and large, it failed to Anglicanize.

As a historian Mr. Cremin recognizes that the many almanacs, newspapers, and magazines published in the colonies had an educational force that is difficult to measure, but he gives them considerable space. He notes that as almanacs were published in great numbers, their authors tended to cease being an academic intelligentsia and became self-educated men, and they included in their pages "the best of English poetry and prose, an infinite variety of essays on topics from anatomy to zoology, a wealth of vigorous political polemic, and every manner of practical advice."

The author devotes a good deal of space to the informal gatherings of men, often undertaken for educational purposes, to which Benjamin Franklin gave the name "juntos." The junto Franklin organized was, in his own words, "the best school of philosophy, morals, and politics that then existed in the province." Mr. Cremin is always conscious that somehow in the second half of the eighteenth century there arose in America a number of broad-gauged men of affairs, many of whom had had a college education; he argues, however, that they began reading political theorists after they had graduated from college, and that, as their "ceaseless self-education" continued, their interests, particularly in history, became more and more secular. "Moreover," he adds, "they read their history, not for diversion or even for self-aggrandizement, but for guidance in the affairs of life."

What Professor Cremin has given us is, I believe, the best history of colonial education that we have yet had. This is not to say that it could not have been better. As often happens when one does something new, Mr. Cremin lays such stress on the new elements that he somehow understates certain other elements: one would like, for instance, to see here more attention paid to particular schools and schoolmasters, but such criticisms can be understressed in the pleasure taken in novel features of the book. In his last substantive chapter Mr. Cremin discusses literacy statistics and concludes that literacy rates in the eighteenth century were "higher in the American colonies than in Ireland, and roughly equivalent as between English and American white males, with the Americans possibly having a slight edge." When the Abbé Raynal, in the face of provincial self-congratulation on the dissemination of education, charged that America had not yet

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produced a man of genius in a single art or science, Thomas Jefferson replied by naming Washington, Franklin, and David Rittenhouse. We have since discovered a few more names that he might have mentioned—Jonathan Edwards, Benjamin Rush, Jefferson himself, James Madison, and—well, everybody can add a name or two.

Mr. Cremin ends this volume by pointing out that the Revolution was both an end and a beginning. He quotes Benjamin Rush, who said in 1787 that "nothing but the first act of the great drama is closed. It remains yet to establish and perfect our new forms of government, and to prepare the principles, morals, and manners of our citizens for these new forms of government after they are established and brought to perfection."

FREDERICK B. TOLLES
Swarthmore, Pennsylvania

DANIEL J. ELAZAR. *Cities of the Prairie: The Metropolitan Frontier and American Politics.* ("Studies in Federalism," sponsored by the Center for the Study of Federalism, Temple University.) New York: Basic Books. 1970. Pp. xiv, 514. $15.00.

Several trends appear in recent studies of the city by social scientists. Efforts are being made to transcend narrow disciplinary boundaries, and the comparative approach is being used to go beyond the merely local. Also emerging is the realization that a historical perspective is necessary to understand the present fully. *Cities of the Prairie* is a good example of all of these. Daniel Elazar's analysis of the political systems of the medium-sized cities of the prairie region emphasizes the historical and cultural context of government and politics. His spatial and temporal comparisons offer useful guidelines to those of us concerned with developing a viable comparative approach to urban history.

The communities examined in detail by Elazar are seventeen cities in five states, particularly several Illinois cities including Champaign-Urbana, Peoria, and Joliet and, for comparative purposes, cities such as Duluth, Minnesota, and Pueblo, Colorado. Designated as "prairie" cities, these communities vary from small empire cities to dormitory satellites in complex metropolitan regions. Elazar's primary research on these cities concentrated on the years from the end of World War II to the Kennedy administration, a period of rapid metropolitanization in this region and in the country.

In establishing a larger setting for his analysis, Elazar devotes a major portion of his book to a provocative discussion of the historical and cultural factors he contends have shaped the political environment of these cities. Four major forces are isolated: the frontier, migration, sectionalism, and federalism. Three frontier stages of American development are outlined, based on an adaptation of Frederick Jackson Turner's frontier thesis. The rural-land frontier of the seventeenth and eighteenth centuries was followed by the urban-industrial frontier of the nineteenth and early twentieth centuries. The growth of the large nineteenth-century city led in turn to a countervailing trend characterized by suburbanization and decentralization, the metropolitan-technological frontier that has become particularly evident since World War II. Elazar's description of the earlier stages would have been strengthened had he taken more seriously the Bridenbaugh and Wade studies of the early urban frontier. His discussion of the third frontier stage, however, offers stimulating new suggestions on the importance of the medium-sized community in urban America. As he points out, most social and political research on American communities has concentrated on the largest and smallest communities, yet fully twenty-five per cent of Americans now live in communities of fifty to two hundred fifty thousand people. Surprisingly, Elazar does not evaluate the extent to which these medium-sized cities are dominated by larger cities. In what ways, for example, does Chicago influence the smaller Illinois cities economically, culturally, and politically?

Urban historians who insist that the city must be examined in relation to its cultural context will appreciate Elazar's efforts in this direction. In a series of bold generalizations, he weighs the effect of the cultural and political traditions of native and foreign migrants on local government. For example, southern Illinois communities have been influenced by the Southern tradition of oligarchy while those in northern Illinois have tended toward Yankee communitarianism. Differences in origins con-

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tinue to survive especially in the form of religious associations that have become vital agencies in transmitting cultures.

Elazar's studies of American federalism are well known. In this book he also effectively analyzes the significance of the federal system for urban government. He takes the current controversy over the decision-making process in cities beyond that of the "power-elitists" such as Robert Hunter and "pluralists" like Robert Dahl by showing the extent to which local political systems form only a part of a larger governmental system. Western cities in particular have felt the impact of higher levels of government. In fact, the success of local government often has depended on the degree to which the local community can take advantage of higher levels of government. Unlike many contemporary commentators, Elazar does not view recent urban decentralization with alarm. In terms of political systems, he concludes that the future is with the essentially "decitified" medium-sized communities, for their size seems to enhance both the possibilities of democracy and efficiency.

Cities of the Prairie is a sophisticated and extremely readable contribution to community studies. By combining detailed local research with a sweeping synthesis of American history, Elazar has successfully illustrated some of the major factors shaping the political systems of American cities.

GILBERT STELTER
Laurentian University

DALE RIEPE. *The Philosophy of India and Its Impact on American Thought.* (Publication Number 772, American Lecture Series. A monograph in the Bannerstone Division of American Lectures in Philosophy.) Springfield, Ill.: Charles C. Thomas. 1970. Pp. xvii, 339.

HAL BRIDGES. *American Mysticism: From William James to Zen.* New York: Harper and Row. 1970. Pp. xi, 208. $5.95.

These competent studies marshal their evidence in a historical or chronological framework with digressions into biography and world views. Both survey American religious philosophy in guidebook fashion, providing students with excellent bibliographies and indexes and essentially supplementing each other. Riepe's account of the impact of Indian thought, both Hindu and Buddhist, on American philosophy—the first extensive effort of its kind—begins in the 1700s and leads directly to the Romantic movement—to Emerson, Thoreau, Alcott, Whitman, the later Transcendentalists, the St. Louis Hegelians, Paul Carus, theosophy, and spiritualism. He next considers the founding of Ramakrishna-Vivekananda centers and other influences preceding 1900, one focus being the World Parliament of Religions in 1893. Harvard University then emerges (1885–1915) as a seminary for Oriental studies in the labors of William James, Charles Lanman, Josiah Royce, Henry Warren, James Woods, Crawford Toy, William E. Hocking, William Bigelow, George Foot Moore, Paul Elmer More, and Irving Babbitt. A chapter entirely on George Santayana precedes one on the key persons who anticipated the Depression years—Edgar Saltus, Ananda K. Coomaraswamy, and James Pratt—and on the earliest Oriental societies. Thereafter the book becomes a series of incisive portraits with philosophical analyses of Orientalists who flourished from the Depression to our own day—perhaps the most valuable section, since Riepe knew most of them personally and through anecdotes. Riepe's discriminating judgment, everywhere apparent, convinces the reader that he himself may be one of the prophets! The conclusion explains why "India has had more influence on American philosophic thought than any other non-Western culture."

Bridges's volume, covering only half a century, is less specialized and more exploratory, an eminently readable analysis of key figures in modern American mysticism less concerned with historians of the subject than with the mystics themselves, from the time of William James (1900) to the present. It recognizes India as only one influence among many on our contemporary manifestation of that "selfless, direct, transcendent, unitive experience of God or ultimate reality," a definition that excludes magic, occultism, spiritualism, parapsychological phenomena, New Thought, and mental healing. Perhaps the first attempt at a unified treatment of the subject in its American setting, Bridges's book is selective, bypassing scientists that show reverence, devout philosophers, and theologians like Hocking and Tillich; literary artists who employ Oriental

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themes as objective correlative (like T. S. Eliot); and contemporaries who interpret the subject in naturalistic terms (as in the school of Sartre). He seeks better criteria than "visions and voices," preferring to be guided in his judgments by the great mystical tradition of earlier ages, of which the American is only a tributary. He says the twentieth century owes a debt to Jonathan Edwards, Emerson, and Whitman.

After considering William James, he deals successively with the leading Quakers, Rufus Jones and Thomas Kelly; with three other members of the Judeo-Christian tradition, Howard Thurman, Abraham Joshua Heschel, and Thomas Merton; with the impact of Vedanta through Aldous Huxley, Gerald Heard, and Christopher Isherwood; with Zen Buddhism as promoted by Shaku, Senzaki, Suzuki, Alan Watts, and Philip Kapleau; and with psychology and psychedelic experience. The last is a timely chapter judiciously weighing what can be said for drugs but denying their significance for genuine mysticism. His evaluation finds the mystical strand in pragmatic America slender but significant, especially on today's college campuses. He combines objectivity with a rich sympathy for his subjects, his style (through quotation from their writings) frequently becoming lyrical as he portrays the pathos of human life and the way in which the mystics redeem or transcend it.

KENNETH WALTER CAMERON
Trinity College,
Hartford, Connecticut

JOHN CALAM. *Parsons and Pedagogues: The S.P.G. Adventure in American Education.* New York: Columbia University Press. 1971. Pp. xi, 249. $10.00.

This carelessly written study proceeds from the dubious premise that the Society for the Propagation of the Gospel was founded in 1701 to stem a welling tide of American republicanism (p. x). This would be done by molding colonists to a replication of traditional British society this side of the Atlantic. In fact the founders of the society seem to have perceived no such aberration, and the author adduces no evidence that such existed. Had the founders done so, they would have been far more perceptive than most Britons of the eighteenth century and many modern historians. Indeed, they seem to have been sincere in their desire "to promote the Glory of God, by the Instruccon [*sic*] of our People in the Christian Religion . . . " (p. 8). Calam committed an elementary methodological error—and by so doing arrived at his operative assumption—when he applied John Adams's famous statement made in 1818 to Hezekiah Niles on "The Real American Revolution" to the period 1714–63 (p. 63). Adams, of course, referred specifically to the years 1760–75.

Whatever their reason for being here, there is no doubt that the missionaries were seen as the semi-official propaganda agents of the British establishment. They preached and taught the High Church opinion of a stratified and highly deferential society with all of its domestic and imperial implications. That such ideas made little impression on Americans is self-evident, and their exponents eventually came to be considered advocates of British tyranny. In attempting an explanation for the lack of S.P.G. success, Calam offers the society's gross ignorance of things American, their innate conservatism and arrogance, and the lack of American support. Unfortunately, it seems to me that he pays too little attention to a weakness evident to both Anglican and non-Anglican commentators. Many colonial Anglican clergymen were failures. Their incompetence had forced them out of the Church of England preferment system to an America where, unable to rise above their lack of ability, they could blame their new failures on local attitudes. The author fails to explain how hundreds of American clergymen succeeded, without complaining, where the English failed. By blaming their reaction on cultural shock the author glosses over their basic incompetence.

Even when focusing on success the author is insufficiently critical. During the period 1701–57, for example, the society distributed about thirteen thousand copies of publications in America (p. 63). This total becomes less imposing when viewed as only 234 per year. Certainly, the conclusion that the society's "mood . . . was unquestionably humanitarian" (p. 213) cannot be squared with the statement on its motive noted above.

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It is unfortunate that this study of such an important facet of pre-Revolutionary intellectual development falls so short in logical, literary, and methodological skills.

GEORGE WILLIAM PILCHER
University of Colorado

HOWARD FAST. *The Crossing*. New York: William Morrow and Company. 1971. Pp. 213. $5.95.

This is a fascinating little book that reminds us of how near to collapse the war of the Revolution had come by Christmas 1776 until hopes were restored by Washington's brilliant Trenton-Princeton campaign. Mr. Fast's focus is on the capture of the Hessians at Trenton. With his usual remarkable craftsmanship he constructs step by step what occurred in the twenty days between the time Washington took refuge across the Delaware until he returned to assault the Hessians.

The Washington one sees here is an impressive leader: farsighted as with Glover and his boats, patient with the discouraging Gates, compelling with certain doubtful subordinates, angry at the tardy Cadwalader, redoubtable in the face of danger. Perhaps on entering the Durham boat for the famous eastward crossing he did indeed nudge General Knox with his toe, and, according to Mr. Fast, said, "Shift that fat ass, Harry—but slowly, or you'll swamp the ——— ——— boat." Perhaps. Even if he didn't, he was too good a leader not to realize the value of relaxing tension among his soaked and freezing troops.

Mr. Fast has written a lively, colorful account. If, at times, he presents scenes and conversations that are at least partially imaginary, they are in context and generally unstrained. Though he has read widely, he has relied a little too heavily upon William Stryker's study of Trenton, good as it is, and Washington Irving's biography of Washington. The latter is a prime source of anecdotes, but should have been balanced with a careful study of Douglas Freeman's more solid study, which does not even appear in the bibliography. On the other hand, if the John Stark he depicts is more cheery than Stark ever was, Colonel Rahl correctly emerges as a more intelligent and formidable opponent than the usual impression of him as a drunken *dummkopf*. There are some excellent notes.

WILLARD M. WALLACE
Wesleyan University

MAX M. MINTZ. *Gouverneur Morris and the American Revolution*. Norman: University of Oklahoma Press. 1970. Pp. xiii, 284. $8.95.

This biography is chiefly a descriptive study of the events in Gouverneur Morris's life, with emphasis on the years between 1776 and 1790. It is not a work that probes deeply into the nature of Morris's ideas about society and government or the influences affecting his philosophy or the motives underlying his activities. Yet even this limited approach stirs the interest of the reader and provokes a great number of questions about Morris's remarkable career. At this distance Morris is scarcely a likeable man. Although he could be charming and witty, he was a snob whose arrogance and dubious tactics eventually won him more enemies than admirers. But, despite these characteristics, his undeniable abilities thrust him early into a position of power in the Continental Congress. There his ideas and recommendations had a strong effect on foreign relations, the management of the army and Western lands, and, as assistant to Robert Morris, the efforts to establish a sound financial and monetary structure for the new nation. As an ardent nationalist he was a sharp critic of the Confederation. It was in the Constitutional Convention that Morris achieved his greatest influence. Perhaps only Madison played a greater role in shaping the document. More than Hamilton, Morris served as the bench mark of high conservative principles. Convinced of the fallibility of mankind and its propensity to do evil, he was dedicated to the rule of the better sort and the supremacy of the property interest in government. Although Morris did not win his way in all matters, it was often his proposals around which arguments were focused and compromises worked out. The arch-conservative revealed surprising flexibility and willingness to accept compromises. He also opposed slavery and took a states' rights position on the question of whether Congress would have the power to negate state laws that contravened the Constitution. And he accepted a document

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that fell short of his requirements for sound government. To all this one must ask why.

Likewise, the last chapter of the book—a short sketch of Morris's life from 1790 to his death in 1816—raises questions. One may wonder what happened to the thinking of a man who so ably argued for a powerful national government and a powerful executive and then supported disunion in 1814. Gouverneur Morris certainly warrants further study.

WILLIAM S. HANNA
University of Oregon

CARL BINGER. *Thomas Jefferson: A Well-Tempered Mind.* New York: W. W. Norton and Company. 1970. Pp. 209. $6.95.

JONATHAN DANIELS. *Ordeal of Ambition: Jefferson, Hamilton, Burr.* Garden City, N. Y.: Doubleday and Company. 1970. Pp. x, 446. $8.95.

These two books share several characteristics. Both authors write in an articulate and attractive style. Both rely largely on sources familiar to the student of the period, and the major contribution of each lies in his interpretation of these sources. Neither author is a professional historian. Binger is a psychiatrist and Daniels a journalist, and the professional insights each brings to his subject are interesting and instructive.

Binger's purpose is to examine the unfolding personality of Jefferson from his birth to the eve of his inauguration as president. Both heritage and environment were important in the development of a genius whose well-balanced personality was a fortunate blend of masculine and feminine traits. The former accounted for his aggressive characteristics, his executive ability, the remarkable precision of his mind. The latter determined his esthetic and artistic temperament, his tenderness and concern for others, his sensitivity and shyness, and his love of solitude. This blend explains much that is otherwise puzzling in his career, such as the internal conflict between the attraction of power and public acclaim on the one hand, and his often expressed wish to retire to private life on the other. He and Hamilton shared the common misfortune of the absence of a father during the critical years of adolescence, which caused both to seek a substitute in an "authoritative figure." Each found this in Washington, causing an inevitable conflict between them as they vied for his love and approval.

Daniels recreates the rivalry that existed among Jefferson, Hamilton, and Burr, seeking to describe both the "companionship and conflict" that characterized their relationship. The emphasis is on the conflict, and the author directs his attention to the "anatomy of their antagonism." He points out that "Jefferson is safe on his pedestal, Hamilton's picture properly graces the American ten-dollar bill," while "Burr, like Lucifer, will never be lifted to the heavens from which he fell, or was 'hurled headlong,'" and he believes that history and many historians have been unjust in their violent denunciations of Burr.

Indeed, Daniels' sympathies seem to lie more with Burr than with either of his two antagonists. Burr shared with Hamilton and Jefferson common characteristics and experiences. All were brilliant and ambitious men who sought political dominance, who were greatly loved and as greatly hated, who fought fiercely with whatever weapon they could command in their conflict with one another, who shared common domestic and economic tragedies, and who were charged, at times unjustly, with shortcomings that ranged from private misconduct to treason.

Both authors characterize Hamilton in a manner that will provide small comfort to his defenders. A ruthless and reckless assassin of character, Hamilton dreamed of power and glory and unhesitatingly used both force and corruption to obtain his objectives. Impatient and arrogant, meddlesome and intolerant, he was also a realist and an apostle of enlightened self-interest, a prophet of industrial America whose energy and creative genius were matched by his conceit and his contempt for democratic idealism.

In contrast to Bender, Daniels finds flaws almost as grave in Jefferson. A sensitive and humorless man, he was, at times, hypocritical, cynical, moralistic, and self-righteous. He, too, could distort facts, could engage in character assassination, could be petty and unforgiving. He worked diligently and successfully to destroy Burr, whom he never forgave for what he judged (wrongfully according to Daniels) to have been Burr's attempt to deprive him of the presidency in 1800. As president he used the

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power of his office to destroy the basis of Burr's political power in New York, eliminate him as a potential successor to the presidency, and finally to charge him with treason and subsequently to hound him both in the United States and in his exile abroad.

Burr is pictured by Daniels as an honorable and attractive man who was betrayed by the Virginia Jeffersonians after he had loyally and effectively aided them in their move to national power. They feared him for his talents and for his popular appeal. Burr was a man far ahead of his contemporaries in his views on democracy; he was concerned with the disfranchised masses in the cities and with the abolition of slavery. Although "no one will ever be able to say with any certainty the exact plotting and planning for a future which went on in Burr's brilliant mind," Daniels seems to accept the thesis that Burr was concerned primarily with land schemes, or perhaps with leading a popular movement against the Spanish in the Southwest. He finds the evidence insufficient to convict him of an attempt to disrupt the Union.

Both books are a welcome and worthwhile addition to the literature of the period.

JAMES L. BUGG, JR.
Old Dominion University

GILBERT L. LYCAN. *Alexander Hamilton & American Foreign Policy: A Design for Greatness.* Norman: University of Oklahoma Press. 1970. Pp. xviii, 459. $9.95.

While this book is the first comprehensive treatment of Hamilton's foreign policy, it offers no new clues and provides no new facts. Instead, it is an extensive piece of apologetics that presents Hamilton in the most favorable light even where the case for him is most fallible. On major points it affirms the argument advanced in the recent briefer study of Helene Looze, *Alexander Hamilton and the British Orientation of American Foreign Policy, 1783–1803* (1969). Lacking the philosophic grasp and psychological penetration of Gerald Stourzh's study, *Alexander Hamilton and the Idea of Republican Government* (1970), Lycan's book glosses over some of the crucial and relevant aspects of Hamilton's behavior.

Thus, Dr. Lycan brushes off Hamilton's derogatory statements about the common man as "products of passing moods" and his vicious personal attack on John Adams during the presidential campaign of 1800 as the result of "temporary gloom." An affirmation that Hamilton's statesmanship "represented a purity of purpose and a high level of ability that is rare in the history of the world" is hard to reconcile with Hamilton's romantic aspirations for personal glory, his propensity for seeking military solutions to both domestic and foreign problems, his special talent for deviousness, and his curious penchant for leaking confidential information to a foreign power. That Hamilton's ends may have been in the national interest hardly justifies the means he used in the area of foreign policy or his undercutting of persons Washington entrusted with foreign affairs missions, notably Gouverneur Morris and John Jay, not to speak of Secretary of State Thomas Jefferson.

Take Dr. Lycan's treatment of the Gouverneur Morris mission. While referring to George Beckwith's report, he astonishingly fails to point out that Hamilton informed Beckwith that he was opposed to discriminatory duties, thereby depriving Morris of the one club he might have wielded effectively, the threat that the United States would adopt high tariffs on British goods. Or take the Jay mission. His advice to George Hammond that the United States would enter into no alliance with the Baltic powers is dismissed as "a serious mistake," thereby implying no ethical judgment on the author's part of what might be regarded as a deliberate misconstruction of Jay's instructions or even less charitably as a betrayal of a state secret. The author presents without comment the evidence of Hamilton's willingness to drop southward the boundary west of the Great Lakes to give Great Britain a direct connection with the Mississippi, while failing to give John Jay his due for refusing to yield this "rectification" to the British. Yielding on this point would have affected the starting point later to be drawn of the boundary westward to the Pacific, with the consequent loss of the greater part of the present states of Minnesota, North Dakota, Montana, Idaho, and Washington—hardly evidence of Hamilton's statesmanlike prescience.

Finally, a reading of Dr. Lycan's treatment of Hamilton's role in the quasi-war with

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France, a period of his most strident imperialism, would hardly prepare us for appraising the high statesmanship of President Adams in winding down an undeclared war—an example with peculiar relevance to our own troubled times.

RICHARD B. MORRIS
Columbia University

DANIEL WALKER HOWE. *The Unitarian Conscience: Harvard Moral Philosophy, 1805–1861.* Cambridge, Mass.: Harvard University Press. 1970. Pp. viii, 398. $15.00.

Some basic themes in American public ethics down to the 1860s are examined in this big book about a small group of Unitarian clergymen and Harvard moral philosophers. Daniel Howe's laudable achievement is the advancement of his subject closer to the front rank of American intellectual history. It deserves to be there partly because Unitarian social thought and action prefigured much that became "progressive" in nineteenth-century American life. They reflected, as well, the modern temper of larger Western culture devoted, as Howe sees it, to capitalism, theism, liberalism, and optimism. More than this, Unitarian moral philosophy deserves primary consideration because it was deeply concerned with the place of rational speculation and academic intellect in a democracy and with some specific themes in Western philosophy. For Howe these moralists successfully combined Burkean prudence and Platonic idealism in their social thought; they were ethical intuitionists, or proponents of a moral sense, in the Scottish Common Sense tradition, inspired particularly by Adam Ferguson, Richard Price, Thomas Reid, Dugald Stewart, and after 1840 by Theodore Simon Jouffroy's developmental idea of moral progress. Considered with the work now being published by Norman S. Fiering, Douglas Sloan, and Donald S. Meyer, this book brings American academic philosophy from 1750 to 1850 out of the "glacial age" to which it too long has been ascribed.

Howe builds well upon earlier scholarship, but his study is original and exceptionally illuminating in three respects. It presents the Unitarian moralists as "archetypal modern intellectuals"; it clearly sorts out the social and philosophical ideas of these last champions of the American Enlightenment, rewarding the reader with a new perspective on the urban reform ideas and efforts of Joseph Tuckerman; and it brings fresh insight to American intellectual history by its examination of the Unitarian common sense esthetic theory and epistemology of art. Howe manages all this by concentrating upon twelve moralists. They generally serve his purposes well. But I am not sure that one can fairly combine Unitarian preachers (John Emery Abbot, Joseph Stevens Buckminster, Joseph Tuckerman, William Ellery Channing) with men who actually taught moral philosophy at Harvard (Levi Frisbie, Levi Hedge, James Walker, Francis Bowen, Henry Ware, Senior and Junior, Andrews Norton, and Edward Tyrell Channing) without including a lot more church and regional history than Howe presents here. The history of Harvard College and of the Unitarian Association are not really identical. Indeed, though chronology in intellectual history is not always easily come by, one misses here a sense of specific timing as to when secondary ideas within Howe's broad "Unitarian consensus" bloomed or faded.

Like many reviewers of good new books in fields they once have worked, I should like to argue with this one as well as praise it. Space limitations allow me only to suggest my thinking. Howe claims that Unitarian moralists began the great withdrawal from doctrinaire theology that characterized almost all of nineteenth-century American Protestantism. Yet the first half of his book tends to obscure this historical drift by emphasizing the schematic and logical, if not doctrinaire aspects of Unitarian theology and ethics. It is, moreover, precisely where Howe sees Scottish Common Sense influencing the Unitarian moralists more than the general Lockean attitude of toleration, empirical reason, and calm observation that I think his study loses its force. Moral philosophy was more a code of conduct for active life than for the study. Howe shows these moralists as keen and occasionally profound thinkers. But in portraying them as eclectic systematizers of various ethical theories, his book skirts the fundamental importance of liberal rationalists surviving in an evangelical and Jacksonian nation. The very nature of their "true" radicalism based upon love of constant learning and their Platonic

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search for the Good made them more than "moral elitists" or even "archetypal intellectuals." It made them, in the best and nonpolitical sense, working democrats, though they would not have liked the label. The history of American intellectuals aiming at an informed republic deserves to be included in any historical assessment of American democracy just as much as the history of mass feeling or of anti-intellectualism.

WILSON SMITH
University of California, Davis

HIRAM M. DRACHE. *The Challenge of the Prairie: Life and Times of Red River Pioneers.* Fargo: North Dakota Institute for Regional Studies. 1970. Pp. xiii, 360. $8.50.

The Challenge of the Prairie will remind many readers of Everett Dick's *The Sod House Frontier*, first published a generation ago. It is the type of social history in which the day-to-day activities of the pioneer generation in the Red River Valley of Minnesota-Dakota are delineated. The life style of these people has been logically compartmentalized under such topics as providing shelter, preparation and cultivation of the land, raising animals on the farm, chores of the housewife, traveling conditions, problems with vagaries of the weather, labor of the family and hired hands, recreation, religion, education, and health and medical practices. In a final chapter the author shifts briefly to basic economics, analyzing land and production costs, grain yield and prices, and taxes and alternative income. From this profit and loss statement he suggests that the major problem of the small farmer was his lack of volume. There is nothing startling about the book's conclusion: "Pioneering in Red River Country, as on any other frontier, was a test of man's endurance. To those who failed to meet the challenge, the area remained a naked prairie, but to those who succeeded, it became a Garden of Eden."

The text is heavily laden with factual evidence, yet it is this myriad of details that sustains the readers' interest, particularly those who are descendants of or identified with the first generation of settlers. A lifetime resident of the region, the author is a farmer, college professor, and radio personality who made his debut as a scholar with *The Day of the Bonanza* (1964), an account of large-scale farming in the Minnesota-Dakota area. In turning his attention to the homesteader he has relied heavily on the diaries, correspondence, and account books of well-known farmers, and the question immediately is raised. "Just how representative of homesteaders in general were these business-like, orderly, record-keeping agrarians?" A somewhat more balanced picture has been achieved and the advantages of oral history demonstrated by extensive interviews with more typical men and women.

The North Dakota Institute for Regional Studies is to be commended for keeping publication standards high in issuing a volume that meets the rigorous criteria set by the professional historian. The bibliography testifies to the author's familiarity with the pertinent literature and his diligence in seeking out the unpublished sources. The extensive illustrations are truly distinctive. In penning a foreword the governor of North Dakota also gives his stamp of approval, thereby undoubtedly enhancing sales.

W. TURRENTINE JACKSON
University of California, Davis

JOHN G. CLARK, editor. *The Frontier Challenge: Responses to the Trans-Mississippi West.* Lawrence: University Press of Kansas. 1971. Pp. vii, 307. $10.00.

These essays honored George L. Anderson upon relinquishing his nineteen-year chairmanship of the history department at the University of Kansas. Sophisticated, analytical, and highly readable, they represent Western historiography in its maturest form. Although the ten papers, originally presented at a conference in Lawrence in October 1969, bear little relation to each other except in their treatment of some phase of the trans-Mississippi frontier, they constitute a fine cross-section of recent approaches to Western history. Since Anderson's writing never dealt with Western stereotypes, the essays—with their diverse subjects and fresh insights—are a highly appropriate tribute.

Anderson's own contribution, "Banks, Mails, and Rails, 1880–1915," considers the way improved transportation facilitated financial transactions. While historians, like Western

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agrarians, have traditionally dwelt on the problem of currency volume in the post-Civil War era, Anderson focuses attention on the great changes in Western financial operations, namely the widespread use of bank checks in lieu of cash and the development of the Railway Mail Service to speed the clearing of checks. He devotes some detail to such fiscal agencies as the country check (actually the problem metropolitan banks had in clearing checks drawn on country banks), the postal savings system, and the postal money order. In these postal operations that involved the flow of paper from local to regional to national offices the author finds seeds of the Federal Reserve System. The responsiveness of the national office to the need for funds in local post offices was another precursor of the banking system. Also, "a system of temporary currency could be maintained partly because all of it would not be presented for payment at one time."

Three essays concern various aspects of Indian policy. Francis Paul Prucha's "American Indian Policy in the 1840's: Visions of Reform" sees that decade as one of relative calm in Indian-white relations. He considers plans for civilizing Indians within the broader context of the reform movement. Despite the fact that not all the goals of educating and Christianizing the Indians were met, substantial advances occurred. In "Indian Allotments preceding the Dawes Act" Paul W. Gates traces from colonial times the practice of allotting specific plots to individual Indians. He notes that during allotment proceedings in 1825 federal officials such as John C. Calhoun participated in the systematic corruption and intoxication of negotiating Indians. By the time of the Dawes Act in 1887 more than seventeen million acres had been allotted. William T. Hagan concludes in "Squaw Men on the Kiowa, Comanche, and Apache Reservation" that squaw men in Oklahoma served as representative precursors of American civilization, with all its enterprise and avarice. Indian agents found squaw men useful in administering reservations as frequently as they were a hindrance. Bent on financial gain, squaw men played major roles in reservation politics and factionalism.

Rodman W. Paul's persuasive essay on "The Spanish-Americans in the Southwest, 1848–1900" considers the differences within this minority in various areas. New Mexico's large Spanish-speaking population consisted mostly of illiterate peons subject to the authority of rich landowners, village headmen, and priests. Arizona, because of the scant Hispanic population, developed a bicultural society, with Tucson as the focal point. Although Anglos were commercial and political leaders, they often married Spanish-speaking women. With California's original Hispanic population of about 7,500 engulfed by the gold rush, it was remarkable that any traces of their civilization remained. In Texas the dominant Anglo culture promoted an abiding distrust of and contempt for the Mexican element. Throughout the Southwest the tide of Anglo civilization easily dominated the indigenous Hispanic residents, especially those low on the social and economic scale.

WALTER RUNDELL, JR.
University of Maryland

JOHN S. WRIGHT. *Lincoln & the Politics of Slavery*. Reno: University of Nevada Press. 1970. Pp. xiii, 215. $6.00.

It was, of course, his involvement in the slavery question that gave Abe Lincoln to history, and Professor Wright may perhaps be excused for probing once again the nature of that relationship. Unfortunately, except for some interesting detail concerning Illinois politics and its bearing on Lincoln's development, most of Wright's story is old hat.

"Lincoln," says Wright, "was neither wholly politician, devoid of deeply felt, substantive goals, nor wholly abolitionist, unconcerned about method and consensus, but rather, the politician of the moral issue." Long persuaded of the immorality of slavery, he became after the Kansas-Nebraska Act "almost a monomaniac on the question of slavery in politics." Although moderate in tone and style, Lincoln advanced progressively more radical doctrines that helped to turn the Republican party into a genuine, if infinitely patient, abolition party. His skill at concealing the radical implications of his creed gave him greater "availability" than Seward or Chase and allowed him, as president, to oversee the destruction of slavery throughout the United States.

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Wright's emphasis upon the growing radicalism of Lincoln's goals is well taken and fits nicely with Eric Foner's recent analysis of early Republican ideology. And Wright's depiction of the political astuteness with which Lincoln pursued those goals is sound if rather conventional. So, too, the author's attention to Illinois politics, and the complexity of that state's response to the slavery crisis helps to set Lincoln's antislavery ideas in context. Yet the social and economic basis of those ideas receives little scrutiny. Despite a swarm of recent studies suggesting the importance of racial beliefs in shaping attitudes toward slavery, for instance, Wright all but ignores the views of Lincoln and other Illinoisans on Negro equality and colonization.

Likewise Wright says little, and nothing new, about Lincoln's perception of the practical threat that slavery—and the Slave Power—posed to all of American society. Certainly slavery was a moral issue. But it attracted a hardheaded politician like Abraham Lincoln precisely because it seemed to impinge so menacingly upon the whole course of national development. By failing to explore the nature of Lincoln's fears on this point Wright seriously curtails the value of his study.

RICHARD H. SEWELL
University of Wisconsin, Madison

STANLEY W. CAMPBELL. *The Slave Catchers: Enforcement of the Fugitive Slave Law, 1850–1860*. Chapel Hill: University of North Carolina Press. 1970. Pp. viii, 236. $8.00.

STANLEY FELDSTEIN. *Once a Slave: The Slaves' View of Slavery*. Introduction by THOMAS P. GOVAN. New York: William Morrow and Company. 1971. Pp. 329. $8.95.

Campbell has made the first comprehensive study of the enforcement of the Fugitive Slave Law of 1850 and challenges the long-accepted position that this law became a dead letter because of a hostile public opinion in the North. In sketching the story of the return of fugitives prior to 1850, he emphasizes the Prigg, Van Zandt, and Latimer cases of 1842 as the turning point that released states from any legal responsibility in this area and asserts that the subsequent personal liberty laws of the Northern states did not prevent a single slave from being returned to bondage. His account of the passage of the act of 1850 would have been improved by some analysis of the vote on the measure.

In a competent and effective manner Campbell discusses the controversy over the constitutionality of the act, the executive determination that the measure be enforced, the state of public opinion both before and after the Kansas-Nebraska bill, and the enforcement of the act from passage to secession. His research in federal and state records has been thorough and extensive, but the question of adequate coverage of newspapers and periodicals might be legitimately raised. Eight of these items were published in Boston and New York; the other four, in New Haven, Philadelphia, Washington, and New Orleans. With the vast majority of the 332 cases (given by year in the appendix) taking place in Pennsylvania, Ohio, Indiana, and Illinois, perhaps more attention should have been given to the press of those states. Campbell does, however, utilize fully the secondary materials dealing with those states.

Interestingly, there were almost as many individuals returned to bondage without due process as were remanded by the federal tribunals (141 to 157). The tribunals released eleven, one escaped, and twenty-two were rescued from federal custody. The rescues and public dissatisfaction with the act were given much publicity, and the constitutionality of the measure was not settled until *Ableman v. Booth* (1859). Though public hostility was not often translated into open opposition to the law, the slaveowners did have some basis for believing that the Northern public had rendered the law ineffectual. Campbell's evidence, however, gives strong support to his contention that the law was faithfully and quite effectively enforced.

The title of Feldstein's book rather accurately describes the work: it is not a history nor is it offered as the true story of slavery. It is simply what the "slaves whose stories were written down *said* about slavery" (p. 15). Feldstein says he has read most of the six thousand extant slave narratives, the great majority of which appeared between 1830 and 1865. His bibliography lists narratives by 133 individuals and thirty-nine other books that contain narra-

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tives. Many of these items are familiar to any student of slavery.

The dust jacket claims this is the first study that, "through the words of the slaves themselves," details the "emotional and physical effect of slavery on black American slaves." Feldstein makes no such claim, but his book shows no indication of knowledge of Charles H. Nichols's *Many Thousand Gone: The Ex-Slaves' Account of Their Bondage and Freedom,* published by E. J. Brill in Leiden in 1963 and later reprinted in paperback by the Indiana University Press. There are nearly a hundred duplications of bibliographical items in the two books, but the approaches of the authors are quite different. Nichols makes more extensive use of the early narratives, presents "composites" from several accounts, frequently weighs the slaves' accounts against other evidence, carries his story beyond the Civil War, and tells a more integrated story.

Feldstein presents his material under seven headings: the beginnings of dehumanization; the area of dehumanization, the plantation; the master's business; types of slaves; attitudes, relationships, and customs; the slaves' interpretation of the institution; and slavery in a civilized society. There are some forty subheadings. The well-chosen selections reveal some diversity of opinion among the slaves, show that the slaves probably understood the master better than the master understood the slaves, and give their reactions to, or opinions of, many facets and details of the system, often in moving and dramatic terms. The author believes this "story told by participants" indicates that for the most part the slave was neither a militant nor a "Sambo," but a "human being forced into accepting a horrendous situation" (p. 282). And this human being, despite his situation, his anger, and his hatred of many American institutions besides slavery, considered himself an American, not an African.

Feldstein uses the *Slaves Narratives* collected by the Federal Writers' Project in the 1930s in much the same manner as he uses the other narratives. Should these narratives be so used? Could they be better and more properly used in an analysis of the lasting psychological effects of the institutions?

Once a Slave, a distillate of the reactions of the most affected and important participants in an accursed institution, is a useful book. The real usefulness of the volume will be destroyed unless its readers heed the caveat of the author.

CHASE C. MOONEY
Indiana University, Bloomington

WILLIAM HANCHETT. *Irish: Charles G. Halpine in Civil War America.* [Syracuse:] Syracuse University Press. 1970. Pp. xv, 208. $8.00.

Charles G. Halpine, born Protestant and privileged in County Meath, Ireland, was tutored well by his clergyman-editor father. At twenty he chose the United States for his stage, having abandoned first a medical and then a legal education and temporarily leaving wife and child behind. For the next eighteen years he frenetically exploited his facile pen, his caustic if often contrived wit, and his professional Irishman's credentials to shoulder his way into prominence in journalism, Irish-American politics, and, as Private Miles O'Reilly, raucous American humor. His craving for some glorious success as an antidote to his sagging self-esteem encountered more frustration than fulfillment. Perhaps only the perennial rescue operations of a forebearing wife enabled this philandering problem-drinker to survive until the hectic Civil War era cast him high into notoriety, New York City politics, and even affluence. But success came too late; not yet thirty-nine, he lost his battle with alcoholism when he accidently killed himself in a desperate effort to bury his craving in chloroform-induced sleep.

Such a man could easily have come to naught in public affairs, even in the land of opportunity Halpine envisioned. Yet he became an intimate of generals, of New York's greatest newspaper men, and of towering political figures. He left, moreover, an extraordinary autobiographical legacy, for his great gift was capacity for timely, instantaneous, and prolific writing.

The author has patiently and sympathetically fashioned a credible portrait of this harried but talented human being. Social history is enriched by so carefully wrought a delineation of the exultation and agony that befell one victim of the success-fever endemic to America's fluid society. The prolific French biographer André Maurois concluded that true

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biography could be written only by the novelist. Some readers may regret that William Hanchett is not a novelist; others may recognize that, at least in part, he is.

THOMAS B. ALEXANDER
University of Missouri, Columbia

FRANK L. KLEMENT. *The Limits of Dissent: Clement L. Vallandigham & the Civil War*. [Lexington:] University Press of Kentucky. 1970. Pp. xii, 351. $10.50.

Clement L. Vallandigham is among the best-known opponents of the greatest of America's many unpopular wars, the War of the Rebellion. Convinced that force could not restore any Union worth saving, the Ohio Democrat vociferously and eloquently called for peace, denounced emancipation, and attacked the Lincoln administration's wartime restrictions on individual liberty. Presenting the first biography of this great Copperhead in one hundred years, offering it at a time when Americans again confront the complex problems of waging war in a democracy that should hold freedom of expression sacred, Frank L. Klement had the opportunity to offer us new insights into our present controversy over dissent.

But despite the promising title, Professor Klement has chosen not to do so. Coming from one of our leading experts on Copperheadism during the Civil War, this study disappoints. Professor Klement displays the careful research and meticulous attention to detail that marks all of his work; it is unlikely that a more thorough study of Vallandigham's Civil War activities could be produced. But aside from sporadic suggestions that Vallandigham's career epitomizes the conservative's reaction to rapid social and political change, the biography lacks a framework. The title notwithstanding, Professor Klement does not explore the issue of wartime dissent. Except for a short and superficial last chapter, he does not relate Vallandigham's actions to the law of sedition or constitutional protections of freedom of expression as they have developed in America. Professor Klement never really grapples with the question of how far dissent may go before it becomes treason, although one may infer he believes that the right to oppose wartime activities of one's government by word should be unlimited as long as the dissenter does not advocate violent resistance. This apparent conviction shapes Professor Klement's attitude toward Vallandigham and his critics. The Ohio Copperhead was, Klement argues, out of step with the nation, tainted by racism, and a rigid ideologue; but his opponents fare even worse. Vallandigham was attacked, writes Professor Klement, by men "in whose hearts a narrow patriotism burned and whose minds equated Republican doctrine with truth and justice."

Professor Klement's apparent partiality to absolute freedom of expression in wartime is attractive to civil libertarians, but his indictment of Vallandigham's persecutors would be more persuasive if he had demonstrated a fuller awareness of differences between the exigencies of a foreign war and a civil war, between a war fought in pursuance of policies believed to be in the national interest and a war fought to preserve the nation itself.

MICHAEL LES BENEDICT
Ohio State University

E. MILBY BURTON. *The Siege of Charleston, 1861–1865*. Columbia: University of South Carolina Press. 1970. Pp. xvii, 373. $9.95.

This is a blow-by-blow detailing of the siege of Charleston from the first shot fired on the *Star of the West* to the final evacuation. The siege lasted 587 days and was a failure, for the fall of the city was due less to the might of the besieger than to the collapse of the Confederacy. The capture of Charleston, an important port and the symbol of the Confederacy, was for many months confidently expected. At one time the blockading fleet had ninety-six vessels; the army, acting with the navy, seldom lacked for manpower. Sumter and other fortifications were subjected to massive fire power; the city itself was heavily shelled—fifteen hundred rounds in one nine-day period—and felt the havoc of destructive fires.

Actually Charleston was in a good defensive position. The harbor defense included forts, powerful batteries on adjacent islands (James Island as a key to the defense is stressed), booms and torpedoes at the harbor entrance, pilings in the shallow parts of the harbor, and batteries on the waterfront. In the rear, entrenchments and breastworks stretched across what was essentially a peninsula, the inland

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waterways were obstructed in various ways, and there were about sixty land batteries. Strategically the situation remained fairly static, but there were constant tactical maneuvers. It is a record of bloody assaults, gallant defenses, inevitable blunders, luck, internal friction, and some remarkable improvisations.

The author, the director of the Charleston Museum and a retired naval officer, has brought together, mainly from original sources, more information on the siege than is to be found elsewhere. It is not a dull narrative; even the trivia heighten the graphic human struggle and sharpen the pity of it all. The author exemplifies his view that "History cannot be changed; it can only be clarified." The focus is on Charleston, but the Union side is fairly presented. It is clear that long after the original Sumter crisis other events around Charleston, so often minimized by historians, were dramatic and decisive. Had the fate of the city been different, the war would have been vastly changed.

Since this is a military account, it is unfortunate that the attractive map used as end papers is not fully legible. If the author had searched additional major libraries, he would have found some extremely interesting material, though perhaps none to change the essential facts. Readers will note with interest that the volume is dedicated to the author's father-in-law, "One of the Heroic Defenders of Charleston."

ROBERT H. WOODY
Duke University

LAWRENCE C. KELLY. *Navajo Roundup: Selected Correspondence of Kit Carson's Expedition against the Navajo, 1863–1865*. Boulder, Colo.: Pruett Publishing Company. 1970. Pp. 192. $8.95.

In 1863 and 1864 the United States carried on a campaign against the Navajo Indians in an attempt to bring to an end the long period of intermittent warfare between them and the whites who threatened their lands and their way of life. The goal was to relocate the Indians on the Pecos River in eastern New Mexico at a place known as the Bosque Redondo, where they were to establish a peaceful, self-supporting agricultural existence. Since the Navajos refused to migrate voluntarily, military force was sent against them.

Lawrence Kelly has made an important contribution to the study of the Navajo expedition in this volume of selected correspondence among Brigadier General James H. Carleton commanding the Department of New Mexico, Colonel Christopher (Kit) Carson of the New Mexico Volunteers who directed the operation, and subordinate officers. By persistent archival digging he has gathered together the essential documents that tell the story of Carson and his expedition and of the "long walk" of the subjugated Navajos to the Bosque Redondo. Nearly one hundred documents are presented, over half of which are published here for the first time. Kelly has tied the documents together with his own narrative and comment, indicating the significance of the reports and the changes they make in the previously accepted accounts of the campaign, and he has supplied extensive footnotes. All students of this chapter in Navajo history will be indebted to him for the thoroughness of his research and the clarity of his presentation.

There are values in the book, as well, for persons interested in broader matters of Indian policy, for the attitudes of the American officials—exemplified best in the reports of General Carleton—were typical of the view that hostile Indians could be subjugated and then quickly transformed into peaceful farmers on limited reservations, for their own benefit as well as for the benefit of the whites who moved onto the rich lands that the Indians gave up. The book gives a well-rounded picture of one brief episode, much in contrast to the one-sided accounts of Indian affairs that have a way of capturing popular favor. We see the problems of the army as well as its victories, humaneness as well as harshness in the treatment of the Indians, and throughout the complexities that plagued those seeking to find answers to the problems of Indian-white relations in the Southwest.

FRANCIS PAUL PRUCHA
Marquette University

RICHARD SENNETT. *Families against the City: Middle Class Homes of Industrial Chicago, 1872–1890*. (Publication of the Joint Center for

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Urban Studies of the Massachusetts Institute of Technology and Harvard University.) Cambridge, Mass.: Harvard University Press. 1970. Pp. x, 258. $8.50.

Richard Sennett's *Families against the City* is a provocative study of the psychological and sociological milieu of the middle-class family—specifically twelve thousand residents of Union Park—that attempts to assess the impact of urbanization and the causes of mobility. Framed within the context of the Ariès-Parsons debate over the influence of industralization on the family, the argument, inspired by Dreiser's *Sister Carrie*, is that these families were "little islands of propriety." Sennett offers three major contentions: first, there was a transition from patriarchal to matriarchal family dominance during the mid-nineteenth century; second, families became isolated, intensive, and repressive, protecting their members from the disorder and diversity of the city: and, third, it was the members of the minority of extended families, not those from the majority of nuclear families, who enjoyed the greatest mobility and, by implication, adapted best to the urban environment.

The great strength of Sennett's essay is his sophisticated definition of the problem, his skillful organization of the argument—he interplays theory and data, literary and quantitative evidence, facts and implications—and the wealth of ideas and insights he suggests in the process. An imaginative "cross-sectional" portrait of the stages of family life, drawn from the data of the 1880 census, indicates that young adults remained in the family well into their twenties, that they married and established their own households only after achieving job status and security, that they rigidly controlled the number of their children, and that there was a high proportion of broken families. Following the theory of "cognitive dissonance," Sennett concludes that these families were obsessed with perceived threats to their economic viability and family cohesion.

Sennett's criticism of these families turns on a comparison of the occupational patterns of workers in nuclear and extended families. He concludes that there was considerable fear of status loss, that fathers were occupationally stagnant, and that the nuclear family cut off its members and failed to prepare its children for work careers in its "flight from urban pluralism." The evidence here simply does not support the argument: occupational mobility is the sole judgmental criterion, the possible influence of other variables in determining career patterns is not considered independently of family structure, and, unfortunately, the number of extended families is too small and their peculiar character too vague to represent an adequate foil to the nuclear families. But it is a thesis well worth pursuing.

More important, the role of the city is never clearly defined, demonstrated, or distinguished from that of the family, and its causative influence is implicit, rather than explicit, throughout the argument. Richard Sennett has hardly proven anything conclusively (this is not his style or purpose); instead, he has provided a provocative introduction to the subject, an original conceptual and methodological approach, and an arresting argument that should greatly enhance the field of urban family studies.

WALTER S. GLAZER
University of Pittsburgh

ROBERT D. MARCUS. *Grand Old Party: Political Structure in the Gilded Age, 1880–1896.* New York: Oxford University Press. 1971. Pp. x, 323. $7.95.

JAMES D. NORRIS and ARTHUR H. SHAFFER, editors. *Politics and Patronage in the Gilded Age: The Correspondence of James A. Garfield and Charles E. Henry.* Madison: State Historical Society of Wisconsin. 1970. Pp. xxix, 304. $7.95.

Poor Matthew Josephson. For three decades historians who have worked over the late nineteenth century have worked over him. Now Robert Marcus delivers a few more blows. Platt and other Stalwarts actively aided Garfield before the Fifth Avenue Conference, whose real aim was to help Platt force Conkling into the campaign. Republicans were short, not long, on funds in 1884, and what they had came more from wealthy politicians than from corporations and businessmen. Similarly in 1888 the tariff issue fried less fat out of Pennsylvania manufactures than Josephson claimed, most money coming from wealthy Republicans like Wanamaker who had no direct stake in

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tariff legislation. The businessman as president maker was a traditional role in Republican politics and not one created by Mark Hanna.

Were this cuffing of Josephson all there is to Marcus's book, it would be only moderately interesting. His main theme is more important: at a time when American society was groping toward increasingly centralized and bureaucratic direction, the structure of the Republican party was becoming more decentralized. Instead of the national committee gradually rising to dominance over state organizations, which Marcus says he expected to find, there were weak national committees with no continuity between campaigns. Power remained in the state organizations. State political leaders calculated each action in national politics by its effect on their control over the state party. Commitments back home and the condition of their organizations for the coming campaign took priority over nominating any particular candidate. One result was that the field of president making fell to amateurs, businessmen like Stephen Elkins and Hanna, men "without local commitments and having neutral non-political avenues by which to seek coalitions," men who "could afford to be gamblers."

In the early 1890s the process was suddenly reversed, but not for the reasons historians have generally assigned. For party members chafing under the tightening control of state bosses, the revolution in voter preference that produced huge Republican majorities created both the opportunity to challenge the bosses and prizes worth the risk. Hanna and McKinley, "the one popular, available, and . . . logical candidate," were the beneficiaries, as these men rallied around the popular Ohioan's candidacy as symbol and focus for their anti-boss campaigns. In the end the bosses beat back the challengers, but, prepared as always to sacrifice potential national power to solidify actual state power, they kept their organizations united by yielding to the presidential preference of their opponents.

That the national committee became an effective force in the campaign that followed had less to do with Hanna's genius than the fact that wealthy Republicans, Gold Democrats, and corporations, frightened by Bryan, poured money into it. This gave the committee leverage with Midwestern organizations that needed funds and enabled it to shed its attitude of dependence on the financially secure Eastern state organizations. In a thoughtful concluding essay not entirely connected to the main body of the work, Marcus offers an explanation why this strengthening of the national committee was ephemeral.

Through a shrewd, sometimes ingenious use of hard-to-come-by evidence Marcus has untangled Republican national politics of the period as no one else has. He is especially good in unraveling the plots and maneuverings in the national conventions, although at times his involvement in this effort distracts the reader from the focus of his work. One wonders, however, why Marcus expected to find other than the decentralization that he did. The very absence of national committee records suggests much about its lack of influence and discontinuity.

Politics and Patronage offers an operational view of a congressman using patronage to maintain his base of power at home and check the pulse of constituents. Dealing with a world of postal clerkships and local political gossip, these often self-serving letters are not unlike those in the manuscripts of a hundred other politicians. The fullness of the collection, however, provides a revealing view of the nature of such political relationships as that of Garfield and Henry. Even as it warmed into friendship neither party ever lost sight of the basis of the relationship and what was expected of each.

HERBERT J. BASS
Temple University

WILLIAM F. HOLMES. *The White Chief: James Kimble Vardaman.* (Southern Biography Series.) Baton Rouge: Louisiana State University Press. 1970. Pp. xiii, 418. $10.95.

James Kimble Vardaman, Mississippi governor and United States senator, was truly an enigma—a race-baiter with ideals of social justice for some groups of underprivileged white men. His hatred of Negroes did not carry over to all minority groups. While he despised Italians, Cubans, and most Orientals, he championed Choctaw Indians and vigorously fought anti-Semitism. Beginning his political career in the 1880s as a disciple of Bourbon Redeemers, he rose to prominence in the 1890s through vi-

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cious denunciations of the Negro. Among the less brutal policies he urged were an end to public education for Negro children and the repeal of the Fourteenth and Fifteenth Amendments. In his gubernatorial campaigns he openly encouraged the lynching of Negroes, but as governor he took considerable pains to prevent lynchings. He achieved for his white constituents many progressive and humane reforms, some of which materially bettered the lot of black Mississippians. He crusaded against and finally brought an end to the barbaric practice of leasing convicts to planters and contractors—a majority of the convicts were of course Negroes. In defiance of the politically influential State Baptist Convention he fought against blue laws as violating individual rights. He despised and denounced evangelical reformers as "cheap showmen." He advocated abolition of capital punishment as morally degrading. All this in late nineteenth-century Mississippi, a land inured to violence.

As a United States senator Vardaman joined Robert La Follette, George Norris, and William E. Borah in pushing for Progressive reforms. At the outbreak of war in 1914 he supported Woodrow Wilson's neutral policy but broke with the president over "armed neutrality." He was one of six senators who voted against the war declaration, and he never ceased to fight the military draft. He opposed the harsh features of the Espionage Act of 1917 and the Sedition Act of 1918. Indeed he opposed all abridgements of free speech during the war. The unpopularity of such a course in Mississippi at the time belies any charge that he was just another "cheap demagogue."

Professor Holmes's account of his subject is an objective one, although it is handicapped by the unavailability of any personal papers of Vardaman. While crediting Vardaman's accomplishments and his courage, he lays bare the man's brutal racism. The author has examined the available sources and seems thoroughly familiar with the intricate details of Mississippi Democratic factional politics, although his story might profit from a more thorough explanation of the mechanics of the political system in the state.

Students familiar with this period of Mississippi political history will find no new interpretations in this study. Some may be disappointed that basic questions about Vardaman are still unanswered. Did he, for instance, truly believe that the Negro posed a real threat to white supremacy in Mississippi, or was this argument merely a demagogic trick? And what caused this late nineteenth-century Bourbon to turn into a neo-Populist? Nonspecialists will find here, however, a compact and lucid account of the spectacular, flamboyant, and Southern "demagogue" whose life will continue to be an enigma to students of American political history.

ALBERT D. KIRWAN
University of Kentucky

D. CLAYTON JAMES. *The Years of MacArthur.* Volume 1, *1880–1941*. Boston: Houghton Mifflin Company. 1970. Pp. xix, 740. $12.50.

In 1918 during a crucial phase in the Meuse-Argonne offensive a tired corps commander visited one of his subordinate generals on the eve of an assault against a key position in the *Kriemhilde* line. "Give me Chatillon," Summerall demanded, "or a list of five thousand casualties." MacArthur's brigade captured the hill, and twenty-four years later, during the desperate days of the first Allied offensive against the Japanese in New Guinea, he remembered these harsh orders. When the time came to send his only American corps commander into battle MacArthur's demands were simple: "Take Buna or do not come back alive."

This episode reveals much about the man, but particularly it suggests the extent to which he was a captive of his own legendary past. In *The Years of MacArthur* Professor James has recaptured this past, examining MacArthur's glittering career with his eyes wide open. Based upon exhaustive research among manuscripts and personal information provided by many of MacArthur's old associates, this perceptive, impartial, and well-written volume is by far the best book-length study to appear on the subject. Previous writers—not all of them biographers or historians—have dealt with the "Fighter for Freedom," the "Soldier-Statesman," the "Rendezvous with Destiny," the "Riddle" of MacArthur, and, inevitably, the "Untold Story."

Unlike these early authors, Professor James does not forget that even the gods were alleged

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to have their weaknesses. As this book tastefully demonstrates, MacArthur always felt a certain contempt for civilian officials who meddled in his domain and a need to speak out on matters that lay well beyond his jurisdiction (*if we were dealing with any lesser light, one* might say "competence"). He learned early to expect special privileges—something, no doubt, his aggressive mother taught him—and his extraordinary rise in the army was due as much to well-placed friends as to his own immense abilities. He was always somewhat aloof, but he knew how to mix well, and his charm was an effective weapon. He was also a brilliant conversationalist. The chief "could charge admission when he is going good," one of his generals wrote home from the Pacific, "and I would be glad to pay."

In the Pacific war MacArthur was paranoiac about the "Marshall Crowd." Here we learn that in France he believed that the Pershing faction was out to get him. He always demonstrated a flair for the flamboyant, a tendency to exaggerate certain of his achievements, and a dramatic quality that caused more than one spectator to think of the stage. In 1919 an admiring journalist compared him to John Barrymore; in 1943 one of his generals described him as "a queer combination of a Sarah Bernhardt." An outspoken man, there were critical times when he chose to remain silent, and Professor James examines in some detail his role in the Billy Mitchell affair and in cutting back the army's early program of mechanization. No one has ever questioned his brilliance or his personal bravery; many have wondered about his intentions and occasionally his veracity.

Not the least interesting facet of the MacArthur personality is his obsession that a high commander "must protect his public image at all costs and must never admit his wrongs." This probably explains his reluctance to fire an incompetent subordinate—it would reflect against the general who had appointed him.

It may even explain MacArthur's *Reminiscences.*

And it certainly gives us good cause to look forward to the appearance of "the years of MacArthur" in the Pacific and Korea.

JAY LUVAAS
Allegheny College

JOSEPH BRANDES, in association with MARTIN DOUGLAS. *Immigrants to Freedom: Jewish Communities in Rural New Jersey since 1882.* (Regional History Series, the American Jewish History Center of the Jewish Theological Seminary of America, Volume 3.) Philadelphia: University of Pennsylvania Press. 1971. Pp. xiii, 424. $12.50.

In 1882 the first rural Jewish settlement in the New World was created when some forty-five immigrant families—almost four hundred men, women, and children—were brought to Alliance in southern New Jersey through the efforts of the Hebrew Emigrant Aid Society. This event opened up a relatively little-known chapter in the history of United States immigration, social history, and the American-Jewish symbiosis.

I am not certain how far-reaching and significant this particular chapter really was. Be that as it may, it was no little thing—so contemporary sources, both Jewish and general, inform us—for refugees from the slums and pogroms of tsarist Russia to become American farmers, thus disproving the stereotype of the Jew as small-town trader and middleman and giving living proof of the faith of the Jews in the dignity of manual labor. Eventually several thousand more followed to settle in this area, some thirty miles south of Philadelphia. Woodbine, founded in 1891 and incorporated in 1903 as an all-Jewish borough, became the site of the Baron de Hirsch Agricultural School. The school, like all the other enterprises, was privately sponsored and subsidized by men such as Baron de Hirsch or the banker Jacob Schiff.

In very great detail the text relates the story of the successes and failures of the colonists—the conflicts with their sponsors; the clash between religious traditions and American reality, between agrarianism and the industrialization necessary to provide additional and seasonal employment; the growth of the poultry industry for the Philadelphia and New York markets; and finally the absorption of Jews from Nazi Germany. Few names or minutiae seem to have been overlooked, making the chapters somewhat repetitious and tedious. The great reliance upon materials culled from local newspapers, especially the Vineland *Evening Journal,* lends local color at the same time that it distorts the reader's perspectives.

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I feel that some of the flaws in the book are the result of poor editing. The research was originally done by Martin Douglas as a doctoral dissertation at the Jewish Theological Seminary of America, and the publication of the text was entrusted to Joseph Brandes. The latter was not able to break out of the dissertational mold and pattern.

FRANK ROSENTHAL
East Los Angeles College

CHARLES MORROW WILSON. *The Commoner: William Jennings Bryan.* Garden City, N. Y.: Doubleday and Company. 1970. Pp. viii, 487. $10.00.

This readable, undemanding biography has as its main virtue its reasonably objective treatment of Bryan, portraying him neither as the protagonist of righteous causes nor as a Canute of fundamentalism, holding back the waves of progress. The author suggests that many of Bryan's apparent vagaries in his later years were the result of creeping diabetes; otherwise, his basic interpretation resembles that of the Hibben biography of 1929, namely that Bryan was an American of the nineteenth century who never came to terms with the twentieth.

The book is not designed for professional historians. "Chapter source notes" at the end make reference to a number of published and unpublished primary and secondary sources, but these do not exhaust the literature; modern assessments by Barnes, Hofstadter, Challener, and Hollingsworth, for example, are not listed. There are no footnotes. Mistakes occur; some for instance, are a reference to the *AHR* as *Journal of the American Historical Society* (p. 469); "Mary Ellen" for Mary Elizabeth Lease, whose birthplace is erroneously given as Ireland (it was Pennsylvania), and to whom is once more attributed the apocryphal "less corn, more hell" statement (p. 137); Richard Crocker for Croker (p. 170); General Weaver as an Ohioan rather than Iowan (p. 176); Grand Isle for Grand Island (p. 216); "Senator Breslow of Kansas" for Bristow (p. 333); Gavrilo Princip as a "Bosnian Serb terrorist" (p. 342). The author did not delve deeply into the period, which leads him to think that Bryan invented the term "trust" in reference to monopolies (pp. 271–72); for the same reason, his two paragraphs discussing gold-silver ratios (p. 112) are a hash, and he makes no reference to Bryan's role in the ratification of the Treaty of Paris of 1899. Wilson gives more detail to Bryan's upbringing and his personal affairs throughout his life than Coletta did but not more than Hibben, and Coletta followed the tortuous path of Bryan's part in the 1912 presidential nomination fight and the New Freedom far more subtly. Since the author devoted more than half the book to the period ending with the election of 1896, such stresses and gaps are nearly inevitable. Except for Bryan's conflict with President Wilson over the *Lusitania* notes, a sense of drama is strangely lacking, even for 1896 or the Scopes trial, perhaps because the author focuses so narrowly on Bryan himself and does not draw a sufficiently serious picture of Bryan's opponents or the context.

This cannot accurately be called a bad book; it rests, in general, upon sources and may provide some readers with a pleasant introduction to Bryan. But historians will prefer the biographies by Coletta, Glad, and Levine and may wonder whether the Commoner's life might not yet hold secrets that only a psychoanalytic biography could disclose—and with them, perhaps, some important aspects, still obscure, of agrarian thought and action, progressivism, and other concerns of the several million Americans who felt that Bryan was, in important ways, their leader.

W. T. K. NUGENT
Indiana University, Bloomington

ARTHUR S. LINK *et al.*, editors. *The Papers of Woodrow Wilson.* Volume 9, *1894–1896.* (Sponsored by the Woodrow Wilson Foundation and Princeton University.) Princeton: Princeton University Press. 1970. Pp. xvi, 612. $15.00.

The months in Wilson's life as a Princeton professor from September 1894 through August 1896 were a harried time of overburned energies and mixed rewards. Pressed by the demands of growing academic celebrity and the need for money to build a new home, Wilson drove himself relentlessly in too many directions, and the strain told. His papers for the period reveal a scholarly career being pulled apart. They begin with eighty-eight pages of tight lecture notes in legal history; record a multitude of occasional essays, public lectures, and addresses; follow the forced labor of his

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worst book—his potboiler on George Washington; affirm the durable intensity of his love for his wife; and end with his first trip abroad, alone, to recover from what the editors call "a small stroke."

Along the way Wilson searched steadily for satisfying new dimensions in an already taut career. While he consistently turned aside feelers about his interest in leaving Princeton, he confessed a strong instinct for executive work. By the mid-nineties his historical writing was clearly running thin. Alert and sympathetic to the fresh ideas of his friend Frederick Jackson Turner, he could state the case for local and regional history with authentic eloquence. But his preoccupation with the eloquent, the romantic, and the vivid combined with an impatience for "the drudgery of sociology" to steer him past the systematic research needed to probe the new themes. Meanwhile his prowess as a student of political institutions, combined with a winning platform style, involved him briefly in reform in 1896. His lectures that winter at Johns Hopkins on modern city government coincided with a furor over Baltimore spoils politics and resulted in his sharing speaking honors with Theodore Roosevelt before a mass protest meeting of aroused citizens. His estimate of the malady of American urban government derived from his earlier studies—the insistence on the organic quality of governmental structures, the mistrust of checks and balances, the stress on expert and expedient leadership—and pointed directly to remedies championed by advanced Progressives a few years later. As Henry W. Bragdon has noted, Wilson's political preferences during the professorial years can hardly be dismissed as conventional conservatism. On the other hand, his concern for national affairs remained episodic and detached. In fact his whole life seemed momentarily to lack a steady focus.

The reviewer gladly waives his right to quibble over set policy and admits unmeasured awe for the industry and skill of the editors. Their command of this exhaustive project seems almost serene.

GEOFFREY BLODGETT
Oberlin College

DAVID QUENTIN VOIGT. *American Baseball.* Volume 2, *From the Commissioners to Continental Expansion.* Norman: University of Oklahoma Press. 1970. Pp. xxii, 350. $7.95.

The second volume of *American Baseball* sustains the lively style and keen insight of the first and makes this an important work for fans as well as devotees of the mounting literature on "our national game." Voigt has approached baseball since the turn of the century in its historical development as a commercial institution with little attention to aspects of the game beyond the major leagues. The author's strength lies in his broad knowledge of professionalism, his emphasis upon investment and huckstering, his recollection of notable incidents and pennant races, his portraits in the era of Ty Cobb, Babe Ruth, Kenesaw Mountain Landis, and Branch Rickey, and his intensive use of guidebooks, interviews, and a few key journals, including the *Sporting News, Baseball Magazine,* and the *Literary Digest.* Considerable reliance is also placed upon the New York *Times,* Congressional investigations, informal histories by baseball reporters, and the statistical notebooks of Lee Allen, historian of the Hall of Fame. As a professor of anthropology and sociology, he ranges widely in his interpretations, many of which are persuasive and of interest to the social historian.

American Baseball, however, is presented as a work of historical scholarship and therefore proves lacking in a number of ways when considered as a contribution to historical writing. The footnoting is mainly restricted to sporting almanacs or journals, the bibliography ignores most social historians, and the author's range of newspaper and magazine research is limited. The impact of technology, urban change, immigrants, and blacks is mentioned in cursory fashion. While there is much of diamond lore of the professional game, the student of baseball as a part of the social scene and of life in industrial society loses by the book's narrowness of concentration. By ignoring the minors, semi-pro, and sandlot baseball, school and college teams, Little League, softball, and the penetration of the game into recreation programs of playgrounds, parks, prisons, camps, military installations, and scores of youth-centered institutions as well as industries, the author deprives the reader of a broad and intensive understanding of the role of sport in modern society. Nor will the reader discover much about the game's impact upon language, humor, or the arts. Perhaps we expect too much. Within its prescribed limits as a study of professional

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sport in terms of baseball's commercial development this proves to be a significant as well as entertaining contribution.

JOHN RICHARDS BETTS
Boston College

L. I. ZUBOK. *Ekspansionistskaia politika SShA v nachale XX veka* [The Expansionist Policy of the USA at the Beginning of the 20th Century]. (Akademiia Nauk SSSR, Institut Vseobshchei Istorii.) Moscow: Izdatel'stvo "Nauka." 1969. Pp. 466.

D. G. NADZHAFOV. *Narod SShA protiv voiny i fashizma, 1933–1939* [The American People against War and Fascism, 1933–1939]. (Akademiia Nauk SSSR, Institut Istorii.) Moscow: Izdatel'stvo "Nauka." 1969. Pp. 466.

V. B. VORONTSOV. *Tikhookeanskaia politika SShA, 1941–1950* [U.S. Policy in the Pacific, 1941–1950]. (Akademiia Nauk SSSR, Institut Narodov Azii.) Moscow: Izdatel'stvo "Nauka," 1967. Pp. 317.

One serious weakness of American diplomatic history has been its unidimensional character, due in part to the linguistic handicap; it is still rather rare to see a diplomatic historian using non-English material. Most writers, orthodox or revisionist, have tended to derive their facts and interpretations from identical—that is, American—sources. This is obviously unfortunate, since diplomatic history deals with more than one country, and no study of it is adequate without a familiarity with foreign scholarship.

The three books under review are recent samples of Soviet scholarship on American foreign relations. They are worth reading if only because they show striking parallels and analogies to American historical writings. Their basic conceptual framework is Marxist-Leninist, but many of their specific themes are comparable to those of revisionist writers in the United States. These and other products of recent Soviet historiography should be taken seriously in order to free the ongoing controversy among American diplomatic historians from much of its parochialism.

To summarize the three volumes in their chronological sequence, Zubok's posthumous work tells the familiar story of America's emergence as an imperialistic world power at the turn of the century. Focusing on the years of Theodore Roosevelt, the book is unique in the attention it gives to the Near East and Africa, where Russian archival documents seem especially abundant, as well as to the Caribbean and the Far East, where the treatment is more conventional. The author sees American expansion as a worldwide phenomenon, although he recognizes that it took different forms in various parts of the globe. The reader has to reach page 411 to obtain the author's clear formulation of "imperialism," which is strictly Leninist, but Zubok also talks about "military feudal imperialism," "economic expansionism," and "ideological expansion," showing parallels to American historiography. Missionaries, businessmen, and diplomats are all viewed as part of the expansionist drive of the country. One of the most interesting things about this voluminous work is that the author almost always accepts reports and interpretations by tsarist officials about American policy. They saw the United States as a world power, usually hostile to the interests of Russia, that was trying to increase its armament, assert its hegemony over the Western hemisphere, cultivate the British wherever practicable, and establish economic control over China—a view that the author shares, as do most American historians. Unlike the latter, however, he is not very interested in studying the decision-making process, and when he talks about public opinion it is often in such vague terms as "the anti-monopoly movement" in the United States, "the mass of people" in Colombia or Morocco, and "the national liberation movement" in China. They are presumably the antithesis of American imperialism, but the book has far more on the intraimperialist struggle in the world arena.

While Zubok's work is fairly conventional in approach and documentation, Nadzhafov's study of American opinion in the 1930s is an ambitious attempt to reinterpret the history of isolationism. The author squarely sets himself up in opposition to most historians who see American foreign policy in the interwar years as a reflection of, if not determined by, isolationist opinion. On the contrary, he argues that "the mass of people" in the United States were heirs to the tradition of democratic struggle against war. Nadzhafov contends that their antiwar sentiment did not make them isolationist but turned them to antifascism. It was the government, not the people, the book says,

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that was isolationist. This was because the former was an instrument of reactionary monopoly capitalism, whereas the latter partook of the worldwide struggle against aggression and fascism. The people saw the difference between aggressor and victim, between right and wrong, and understood that their salvation lay in identifying themselves with the democratic tides sweeping across the entire globe. They were for resistance to fascism abroad and struggle for freedom at home. The author traces the evolution of this movement through 1939, as it passed from the stage of the popular front to that of the democratic front. Quite predictably he stresses the role of the Comintern and the Soviet Union, and he relies heavily on Communist sources, American as well as Russian, for documentation on American opinion. At the same time he criticizes American historians for underestimating the antiwar, antifascist sentiment of the "progressive" people and failing to see that official policy was basically "egoistic," expressing the interests of monopoly capitalists.

Vorontsov is less critical of Franklin D. Roosevelt than Nadzhafov. His essential points should sound familiar to revisionist writers in the United States. Although he commends Roosevelt's "realistic policy" during the war, which, among other things, produced the Yalta agreements, the author basically interprets the Pacific war as providing an opportunity for American imperialism to wrest control from the European colonial powers in Asia. The Atlantic Charter, when applied to Asia and the Pacific, meant a formula for entrenching American economic interests in the European colonial regions. The trusteeship idea, the book argues, was but a scheme to achieve this objective. War in Asia could be utilized by the United States to spread its ideological and economic influence, to turn it into America's sphere of interests. The competition against the European imperialists, however, was compromised because the United States shared their fear of national-liberation movements among the colonial populations. Only the Soviet Union supported the latter wholeheartedly. Hence the inevitable rift between Russia and America after Soviet entry into the Pacific war, which served to strengthen democratic forces in China and elsewhere. Balked in the attempt to dominate Southeast Asia, the American imperialists sought to use China as a stronghold against the Soviet Union and the national-liberation movements throughout Asia. When this, too, was frustrated by Chinese nationalism, they established control over Japan. The Korean War was essentially part of the worldwide struggle between imperialism and the national-liberation movement.

The three authors show familiarity with American documents and historiography, although there are notable gaps. Vorontsov also uses Chinese and Japanese material. In addition these works share two basic characteristics: the tendency to view foreign policy as an integral part of domestic social developments, and the global perspective, seeing American diplomacy in relation to events throughout the world. The first, of course, also characterizes American writings, as they, too, have been concerned with "domestic sources of foreign policy." It cannot be said that either Soviet or American historians have entirely succeeded in their attempt. Soviet writers are especially weak when they try to identify social classes such as laborers and farmers with fixed attitudes toward problems of foreign relations. Quite often "progressive" views expressed by members of "monopoly capitalism" are dismissed as opportunistic expediencies, while "reactionary" attitudes of working classes are attributed to their inability to understand their "objective" situation.

The stress of the Soviet historians on global developments has much to commend it, however, especially since such a perspective is often lacking in American writings. It sometimes results in oversimplification, as when the three authors under review lump together England, France, tsarist Russia, Germany, and the United States as imperialists, or Nazi Germany and militarist Japan as fascists, as if such designation were sufficient in defining their common interests and outlooks. Nevertheless, by calling attention to the interests and policies of other governments and peoples, these books add an extra dimension to the study of American foreign relations. They demonstrate that these relations must be looked at from the outside as well as from the inside, and that no historiographical debate within the United States is

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adequate without further interchanges of ideas between American and foreign specialists.

AKIRA IRIYE
University of Chicago

JEAN B. QUANDT. *From the Small Town to the Great Community: The Social Thought of Progressive Intellectuals.* New Brunswick: Rutgers University Press. 1970. Pp. vii, 260. $9.00.

Recent scholarship has provided us with a number of perceptive insights and new interpretations of the Progressive era. A case in point is Jean B. Quandt's imaginative study of the social thought of nine progressive intellectuals and reformers: the sociologists Robert E. Park, Charles Horton Cooley, and Franklin H. Giddings; the philosophers John Dewey and Josiah Royce; the urban reformers Mary Parker Follett, Jane Addams, and Frederic C. Howe; and a journalist, William Allen White. It would not ordinarily occur to most historians to lump this mixed group together, but Professor Quandt argues persuasively that their interest in community represented a special aspect of American thought in the early twentieth century. These communitarians shared both small-town origins and a mounting concern over the impersonality and estrangement characteristic of modern urban industrial society. Recalling nostalgically the sense of solidarity and the shared values of the rural and small-town communities of their youth, these nine communitarian Progressives (and others whose ideas are explored less extensively) sought to create a wider community that would restore the small-town unity of spirit and purpose in an America increasingly characterized by complex, specialized technology and sprawling, balkanized cities. They recognized and worried over the paradox that as Americans were becoming ever more interdependent they were losing their sense of common identity and purpose. What was needed was to re-create the shared experience and unified spirit of the small-town community in ways supportive of the larger community of city, state, and nation.

The communitarian Progressives believed it possible to establish the wider community through a variety of means. Within the cities the settlement houses and other neighborhood groups could mitigate individual loneliness and isolation and provide the basis for group action and interaction. The revolution in mass communications made it possible for town-meeting democracy to function at even the national level and to extend the ties of family and neighborhood to the whole society. The impersonality and boredom of the assembly line with its minute division of labor could be reduced by educating workers to appreciate the value of their work through seeing the end product and understanding the historical development of machine technology. At the same time they might be taught arts and crafts so they could feel more creative and take pride in their work skills. The increasing specialization of knowledge might similarly be countered by encouraging scholars to concern themselves with large problems studied from a broad interdisciplinary perspective. In short, the tools were at hand to extend the face-to-face unifying virtues of the small community to all levels and aspects of the larger society.

Professor Quandt's study is not without flaws, however. She has not satisfactorily resolved the organizational problems inherent in trying to deal separately with individual thinkers while at the same time exploring broad themes of interest to the communitarians as a group. As a result some of the discussion is diffuse, rambling, and repetitive. The basis for selecting this sample of nine communitarian Progressives is never made clear, nor is there much effort to relate them to each other or to the various other thinkers who are discussed somewhat haphazardly throughout. Still, this is a promising first book, which may be read with profit by students of the Progressive movement.

E. DAVID CRONON
University of Wisconsin, Madison

F. GARVIN DAVENPORT, JR. *The Myth of Southern History: Historical Consciousness in Twentieth-Century Southern Literature.* Nashville: Vanderbilt University Press. 1970. Pp. xi, 212. $7.95.

The perennial interest in the South's relationship to the rest of the nation has heightened in recent years because of the dissipation of the old and comforting myths of American virtue, opportunity, and invincibility. The South's historic role as counterpoint to national claims and images has consequently taken on a special meaning as the region is pressed into service to

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provide guides for the ways in which racial and urban problems may be resolved and a sense of community preserved.

Mr. Davenport's monograph contributes to a better understanding of the thought processes that have created the South as both model and counterpoint. It is not, unfortunately, a comprehensive study of the phenomenon but, rather, a series of five essays illuminating aspects of the story in the twentieth century. He traces the dynamics of two strands of what he calls "the myth of Southern history." Beginning with Thomas Dixon, proceeding through the Vanderbilt Agrarians, and culminating with George Wallace, he explores aspects of the myth of white supremacy. With Dixon the burden of Negro equality and liberty threatened the hopes of national progress and sectional reconciliation; with the Agrarians the status quo in race relations is assumed as necessary to the defense of order and community against chaos and fragmentation; with George Wallace it becomes white backlash.

The second strand, on which Mr. Davenport concentrates more extensively, ties together the emergence and refinement of an ironic view of history in the writings of William Faulkner, Robert Penn Warren, and C. Vann Woodward. Without departing from the conventional wisdom, he cogently describes a complex and organic sense of history, infused with an awareness of imperfection and human suffering out of which might come the kind of responsibility and moral commitment necessary to redeem the region and, through it, the nation. In the final chapter Mr. Davenport reports the failure of the white South after 1954 to seize the opportunity for wise and humane action and concludes with the judgment that Martin Luther King, Jr. became the inheritor and transmitter of the message of redemption. "The myth of Southern history," he writes, "suggests that out of suffering comes wisdom and that out of wisdom comes the ability to redeem American life. The nonviolent, love-centered quest for justice of Martin Luther King stood in the eyes of many as one of the most profound contributions to this national redemption made by any Southerner in the nation's history" (p. 192). Unfortunately, Mr. Davenport does not carry his analysis beyond King, and therefore denies himself the opportunity of coming to terms with the ironic rejection by much of black America of the role of redeemer of white America.

PAUL M. GASTON
University of Virginia

THEODORA KROEBER. *Alfred Kroeber: A Personal Configuration*. Berkeley and Los Angeles: University of California Press. 1970. Pp. xi, 292. $7.95.

Mrs. Kroeber has already demonstrated in her story of *Ishi in Two Worlds* a gift for an evocative and moving descriptive writing of a high order. There are numerous passages in her "personal, but not intimate" treatment of the life of her husband that reflect this gift: perhaps the most moving is the epilogue describing Kroeber's death in a Paris hotel room. Beyond this there are major sections of the book that do much to illuminate the life of one of the several most influential American anthropologists of this century, who for a decade and a half after Franz Boas's death held a somewhat similar position of dominance in the profession. I have in mind especially the accounts of Kroeber's childhood and of the years of personal tragedy and disorientation that led him into psychoanalysis and almost out of anthropology. As intellectual biography, however, the book has serious weaknesses, which may be symbolized by the identification of "the first scientist to excite and impress" Kroeber as John, rather than Francis, Galton. Mrs. Kroeber tells us that she began her work under the title "Biographical Notes" and changed it only when she found that one could not write of Kroeber without becoming "aware of the pattern and the configuration" of his life and personality. Nevertheless, the last portions of the book, with their numerous abrupt transitions from one topic to another and their somewhat randomly selected passages from Kroeber's later correspondence convey rather more the impression of patchwork quilt than the warp and woof of pattern. Although the preface suggests a different intention, much of the value of the book lies in the anecdotal and the colorful, offered from a perspective no other biographer can hope to duplicate. Kroeber in his seventies festooning himself and family with crepe streamers found lying in the street, and then stretching a paper barrier across it, watching in

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delight as car after car came to a stop and turned around—there is anecdote, and color, and illumination.

GEORGE W. STOCKING, JR.
University of Chicago

WILLARD B. GATEWOOD, JR. *Theodore Roosevelt and the Art of Controversy: Episodes of the White House Years.* Baton Rouge: Louisiana State University Press. 1970. Pp. vii, 294. $8.95.

In this book Professor Gatewood offers essays on seven controversies that punctuated the presidency of Theodore Roosevelt: the Booker T. Washington dinner; the Indianola, Mississippi, Post Office affair; the appointment of Dr. William D. Crum as collector for the port of Charleston; the dismissal of William A. Miller from the Government Printing Office; the diplomatic meddling of Roosevelt's friends, Ambassador Bellamy Storers and his wife, in the affairs of the Vatican; the agitation arising over the deletion of the motto "In God We Trust" on the new coinage struck by Saint-Gaudens; and the controversy in 1908–09 over the proper role of the Secret Service. The author does not give us any new interpretation of TR, as he readily admits, but attempts to "reveal something of significance about Roosevelt as a man and a political leader" by devoting close attention to these case studies of Roosevelt in the midst of controversial political issues. The research is admirably thorough, and we are not likely to need or want more definitive treatment of these controversies. Some of them may seem trivial and tedious, but it is useful to be reminded that in those years the American public, or at least the national press and the Washington political community, could afford the luxury of absorption in issues of such basic insignificance. And certainly it is useful to have a more detailed look at the three racial controversies—the most interesting of the seven—where Roosevelt accidentally and ruefully collided with the incredible bigotry of the day. These sketches do not alter our view of Roosevelt in any important way, unless they suggest that he was even less combative than we thought. He did not relish most of these controversies, and while he turned one or two to his political advantage, he appears generally to have been heavily burdened by the inelegant and inconsequential little political flaps that pressed upon the presidential office. As we know, Roosevelt in public usually took the high ground of principle, but again in this book we see that he had usually given a good deal of prior effort to finding the most expeditious way to avoid being hurt. Readers will not find that this book either offers a comprehensive view of TR or undermines the one we have, but many historians will welcome such meticulous scholarship and the consequent broadening of our knowledge of the presidency of the Republican Roosevelt.

OTIS L. GRAHAM, JR.
University of California, Santa Barbara

RALPH STONE. *The Irreconcilables: The Fight against the League of Nations.* [Lexington:] University Press of Kentucky. 1970. Pp. 208. $9.95.

Most accounts of the defeat of the Versailles treaty have concentrated on explaining why those senators who said they favored ratification failed to agree. The sixteen senators who opposed ratification even with reservations appear in general histories as the "Irreconcilables" or the "Battalion of Death," but, except for the speech-making tours of William E. Borah, Hiram W. Johnson, and James A. Reed, their activities have received relatively little attention. Professor Stone has discovered that the Irreconcilables were far more influential in the final decision than many historians have heretofore believed. They maneuvered adroitly to prevent the only development that could have made ratification possible—a compromise between the reservationists and President Wilson's supporters.

Stone is as interested in the reasons why the sixteen senators took a common stand as in their roles during the treaty struggle. He writes that they were united "only in their conviction that the Treaty must be defeated, not in the nature of their conviction, nor in the degree to which they opposed it." Borah, Johnson, and Reed were the most extreme Irreconcilables, consistently advocating "traditional isolationist-nationalist policies." Robert M. La Follette, George W. Norris, Asle J. Gronna, and James I. France were "philosophical optimists" who at times advocated plans for international organization based upon good will and morality.

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At one time during the treaty struggle France proposed a federation of all nations based upon popular control, but none of the four would support any plan that would give a world organization armed force. They feared that the League would ultimately have that kind of force. Like Borah, Johnson, and Reed, the remaining nine Irreconcilables were ardent nationalists, but they did not advocate extreme isolationism. Philander C. Knox, Miles Poindexter, Frank B. Brandegee, George H. Moses, Joseph Medill McCormick, Laurence Y. Sherman, Albert B. Fall, Bert M. Fernald, and Charles S. Thomas had no confidence in any plan for world organization relying upon good will and morality. The League and the ideas of the philosophical optimists seemed visionary to them, but some members of this group were willing to consider limited agreements of a military nature with America's wartime associates. Knox's views are of particular interest. As skeptical as other members of this group about the real worth of the League, he nevertheless urged compromise. He advised separation of the Covenant from the treaty and ratification of what was left. The United States would continue to cooperate with the European powers to maintain peace. At a later time there could be fuller discussion of the League question.

This book will be of most interest to students of ideas about international relations and to scholars interested in the treaty struggle, but students of political institutions should also find it of particular value. As everyone who tries to write about a legislative or deliberative body soon learns, finding an approach can be difficult. Stone's handling of his topic may suggest solutions to similar problems. And his work should win professional approval for other reasons. He carefully analyzes opinions, but he does not fail to remember that he is writing about human beings. He portrays personalities clearly, and he recounts incidents with considerable narrative skill. This book could serve as a model monograph for many beginning historians.

CALVIN D. DAVIS
Duke University

JACKSON K. PUTNAM. *Old-Age Politics in California: From Richardson to Reagan.* Stanford: Stanford University Press. 1970. Pp. 211. $7.50.

This study, a revision of a doctoral thesis, moves rapidly from Governor F. W. Richardson (1923–27) to emphasize the years of EPIC (Upton Sinclair's plan to End Poverty in California), the Townsend plan, and the Ham and Eggs plan (thirty dollars every Thursday), 1933–40: a final chapter describes the one major pension politician since the 1930s, George McClain, and summarizes changes in benefits to the aged and surveys their problems. Putnam has drawn extensively on newspapers, periodicals, and government documents in developing a critical account of the rise of pressure groups, responses of politicians, and expansion of state programs for old-age assistance. He corrects old errors, including some perpetrated by Dr. Townsend; he breaks new ground with his discussion of the role of the Fraternal Order of Eagles.

The most serious shortcoming of the study is that it is chiefly external, without the insights into motives and attitudes that might have come from manuscript and other private evidence. In the first chapter on the plight of old people in the 1920s and 1930s, Putnam draws liberally on theories of social scientists but makes too many unsubstantiated generalizations. "Millions of 'Okies' and other displaced persons" probably could not have fled to California during the depression (p. 2), when population increased by only 1,230,136 (1930–40), even if "in-migration rather than natural reproduction" accounted for most of the increase. Is there evidence that "the fact that many of California's aged were migrants and ex-Midwesterners tended to increase their propensity toward political activism" (p. 8)? Poorhouses were bad enough without identifying them with county hospitals (pp. 15–16, 149), which by 1930 were separate over much of the state.

The footnotes are informative though difficult to use: some are at bottoms of pages but most in an appendix (pp. 145–87), where citations to government documents appear in abbreviations (U.S. [1–20] and Calif. [1–53]) that refer to the bibliography.

EARL POMEROY
University of Oregon

MARVIN SCHICK. *Learned Hand's Court.* Baltimore: Johns Hopkins Press. 1970. Pp. xvi, 371. $12.50.

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This book embodies a kind of paradox: it rests for inspiration upon the myth, widely propagated in legal circles, that Judge Learned Hand was a man of extraordinary genius. Yet as the author's analysis succeeds rather nicely in demonstrating, the "Learned Hand legend" rests upon a decidedly frail platform of historical reality.

Hand was unquestionably a judge of very considerable abilities. His career as a federal judge extended over a period of more than fifty years, from his appointment as a district judge by Taft in 1909, to his promotion in 1924 by Calvin Coolidge to the United States Court of Appeals for the Second Circuit at New York, to his death in 1961 after nearly forty years of service on the latter tribunal. The present work deals in particular with the work of the second circuit court between 1941 and 1951 when Hand presided over that tribunal as chief judge, at a time when his astonishing reputation already was visibly flowering. In 1947, for example, Justice Felix Frankfurter could write with some alarm that "Learned Hand is headed straight for the glory and dangers of a legend," a development that Frankfurter feared would obscure what he considered to be the very real importance of Hand and his work.

But why Hand, like Oliver Wendell Holmes, Jr. should have been enveloped by his contemporaries in so peculiar an aura of fame is, as the author demonstrates, now more than a little difficult to perceive. Federal intermediate appellate courts, unlike the Supreme Court, have but little impact upon the course of the nation's constitutional, legal, and political development. They must be obedient to the precepts of the law as expounded by the supreme tribunal above them, and any undue exercise in creative originality on the part of the presiding judges invites reversal or even rebuke. The second circuit court during Learned Hand's years was no exception to this rule. The consequence is that an examination of the five-thousand-odd opinions that Hand wrote in the course of his career on the federal bench leads to the conclusion that they contributed only a modicum of original legal thought and that Hand had only a modest impact upon the development of American law, except perhaps in the field of patent law.

Like Holmes, Hand drew a portion of his reputation from his radically restraintist stance on judicial review. He was a consistent proponent of judicial self-restraint; indeed, he actually repudiated the doctrine of judicial review of congressional enactments except in the indispensable matter of statutory interpretation. Yet judicial self-restraint is in itself a rather fragile thing upon which to build so fabulous a reputation. Again, Hand had something of a reputation as a libertarian—his desertion to the Progressive party in 1912 apparently cost him an appointment to the Supreme Court a decade or so later. But in fact Hand's opinions touched very little upon the new law of civil liberties, which after 1940 had an explosive impact upon constitutional development. On the contrary his "sliding scale" theory concerning the extent of restraint upon dissident political activity permissible under the First Amendment, which he enunciated in the Dennis case, came dangerously close to a reformulation of the malodorous "bad tendency" doctrine, although the present author insists upon a carefully drawn distinction. It remains true, nonetheless, that Hand was a consummate legal craftsman, who, as the author says, consistently explored to the uttermost the hidden philosophical and juridical implications of each case that came before him.

The bench of the second circuit court in the 1940s, as the author makes clear, amounted to far more than Learned Hand. Every one of the other five judges—Thomas Swan and Charles Clark, both former deans of the Yale Law School; Harrie Chase, a taciturn but capable Vermont Republican; Augustus Hand, the chief judge's cousin and close collaborator; and above all the brilliant, contentious, and controversial Jerome Frank, New Deal politician and legal philosopher—was a man of some considerable legal skill and professional eminence. In the last analysis it was Frank and not Learned Hand who among the second circuit judges made the most incisive contribution to American legal and constitutional development, notably in the transformation of the constitutional law of criminal procedure over which the Warren Court, with its Escobedo, Miranda, and Wainwright decisions, presided after 1960. Perhaps, one might conclude, the Learned Hand legend deserves to die and be

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replaced by a more substantial historical truth, recognition of the significant role that Jerome Frank played in the formulation of the constitutional law of the late twentieth century.

ALFRED H. KELLY
Wayne State University

EDWARD ROBB ELLIS. *A Nation in Torment: The Great American Depression, 1929–1939.* New York: Coward-McCann. 1970. Pp. 576. $10.00.

JORDAN A. SCHWARZ. *The Interregnum of Despair: Hoover, Congress, and the Depression.* Urbana: University of Illinois Press. 1970. Pp. ix, 281. $7.95.

RAYMOND WOLTERS. *Negroes and the Great Depression: The Problem of Economic Recovery.* (Contributions in American History, Number 6.) Westport, Conn.: Greenwood Publishing Corporation. 1970. Pp. xvii, 398. $13.50.

These three books add to the growing shelf of studies that reveal the misery of the depression years. Without necessarily intending to, they also expose the clumsiness with which the federal government attempted to alleviate the situation. While none of the authors writes from a New Left perspective, the effect of their labors is to underscore the callousness of American society in the 1930s and to raise anew questions about the wisdom of the New Deal.

Ellis, a veteran journalist, says he is writing "on the people themselves. . . . How did they feel? What did they think? What did they do? What did they say? How did they survive?" He writes smoothly, his research in secondary sources is adequate, and his judgments on the New Deal are usually persuasive. Some of his topical chapters—on the dust bowl, the Bonus Army, and the so-called general strike in San Francisco—admirably realize his purpose. By skillfully juxtaposing scenes of misery and opulence he effectively exposes the willful cruelty of the period. But Ellis sometimes panders to his readers; his analyses are superficial; and his focus on such "people" as Samuel Insull, Huey Long, Father Coughlin, and Howard Scott offers little more than colorful interludes. Ellis also finds it hard to resist anecdotal tidbits, some of which add nothing to his story. This is a competent, humane narrative that will not interest scholars of the period, but it might catch on for more general readers.

Schwarz, by contrast, is a scholar who has visited thirty-two archives and read widely in other sources. His account of Hoover's handling of Congress, while critical, is fair. Indeed, he shows that Congress prior to April 1932 was at least as confused as the president. He also refutes the notion, spread by Hoover himself, that Congress was unfailingly obstructive; on the contrary, Schwarz argues, cooperative Democrats like Joseph Robinson and John Garner helped Hoover achieve his limited aims until early 1932. It was only later in that year—after rank and file Democrats rebelled against a proposed federal sales tax—that Congress consistently defied the president. This interpretation, while hardly astonishing, is persuasive, and the chapters on the battles of early 1932 are therefore useful additions to the literature on the period.

Otherwise, the book is disappointing. Far from covering the entire period in equal depth, Schwarz skips rather quickly over the events prior to 1932, and his account of the campaign of 1932 seems irrelevant. At times, especially in the earlier chapters, his unwillingness to discard his notes leads him to include extraneous material and to ramble about. Schwarz also writes unevenly. Thus he talks of the "inevitable 1932 consequences" and of "the substitute-bill Democrats"; and he says that "the depression ineluctably lurked prominently in the background." Above all, Schwarz is sketchy in explaining what some of the legislation actually attempted to do. Why did Hoover accept a revised version of what became the Emergency Relief and Construction Act of 1932? What were the terms of the Glass-Steagall Act? Though Schwarz devotes a detailed chapter to the rebellion against the sales tax, he says practically nothing about the crucially important income tax rates that emerged from the fracas. Above all, Schwarz is reluctant to embark on a sustained analysis of Congress. Though treating the reader to countless roll calls, he seldom attempts to analyze them or to break either house down into groups or factions. Thus while one learns that men like Costigan, Wagner, and La Follette opposed the president in 1932, one discovers little about their followers. Did Congress split along sectional lines? Was it a battle between rural and urban types? Did the senators who had to face the voters in 1932 behave differently from those who did not? Did

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age make any difference? Were there no steady patterns at all? Was the House more or less obstructive than the Senate? Why or why not? By focusing on legislative maneuvering Schwarz misses a chance to provide answers to these important questions.

Wolters's book is really three in one. The first part competently covers already familiar ground in showing the sad effects of the AAA on black tenant farmers in the South. The third part, which is based on careful research in NAACP files, narrates the in-fighting that divided the NAACP between 1933 and 1936. In the process Wolters offers a temperate but convincing critique of Walter White and the moderates, arguing that the NAACP should have pressed harder for economic reform instead of throwing so much energy into the battle for an antilynching law.

By far the most significant portion of the book, however, is the seventy-page chapter in the second part on wages and hours under the NRA. Here Wolters shows clearly how the well-meaning New Dealers usually ended by harming blacks more than they helped. Many employers simply evaded the NRA. Others applied all sorts of ruses to discriminate against blacks. Employers who tried to abide by the codes often found it necessary to lay Negroes off or to turn to labor-saving machinery. And when the codes succeeded in raising prices, it was the poverty-stricken black consumers who suffered most. Wolters seems entirely at home in writing about the complicated rules and administrative problems of the NRA, he uses examples tellingly, and he brings to the story precisely the right mixture of detachment and criticism. The result is a solid case study of the difficulties inherent in national planning of the sort implicit in the NRA and a persuasive brief—so far as blacks are concerned—for large-scale public spending as the best route to social justice.

Because Wolters has limited himself primarily to research on the AAA, NRA, and NAACP between 1933 and 1936, he does not begin to cover the broader subject of "Negroes and the Great Depression." Indeed, he admits that he is not trying to study New Deal relief policies. Thus other scholars moving into this relatively unplowed territory may emerge with interpretations somewhat more favorable to the New Deal. Until they do, however, Wolters's monograph, especially part 2, will remain a book that students interested in Negroes in the 1930s will have to consult.

JAMES T. PATTERSON
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EDWARD N. KEARNY. *Thurman Arnold, Social Critic: The Satirical Challenge to Orthodoxy.* Albuquerque: University of New Mexico Press. 1970. Pp. 164. $6.95.

Historians of the New Deal have acknowledged the particular importance of Thurman Arnold in the formation of social and economic ideas of the period, pausing usually to ponder the relation between his positions in *The Symbols of Government* (1935) and *The Folklore of Capitalism* (1937) and his career as assistant attorney general in charge of antitrust activities of the federal government. Ellis Hawley's excellent analysis, much in evidence in this book, formulates the puzzle succinctly: "in spite of his sly ridicule of the capitalist system, Arnold was apparently an intense believer in a competitive economy and in the idea that such an economy had never had a chance." So another descendant of Veblenian criticism bites the dust, having been drawn into the ambush by the promise of a new treasure on an old map.

Mr. Kearny's book goes further in its intellectual wrestling with the larger proportions of the problem than many historians will feel comfortable in going, but the try may be worth the effort. It isn't really enough—less than 150 very expensive pages for what should have been a compact article or a more comprehensive biography—but it helps.

The opening chapter is a clear enough tour through late nineteenth- and early twentieth-century social thought in America, the sort of thing one wishes undergraduates might read as a base for more thorough analysis. Arnold may look a bit odd on a pedestal built out of the ideas of William James, Thorstein Veblen, and John Dewey; but that may indeed be where he belongs. The plausibility of the perch would have been enhanced by more real research in the chapter on Arnold's background, values, and method. Instead, we get two short paragraphs on his education at Princeton, one on Harvard Law School from 1911 to 1914—the years, obviously, when the Progressive move-

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ment might have been having some effect on a young law student with Arnold's interests. Less than a page later he is accepting an appointment to the faculty of Yale's law school, presumably because he had "developed a habit of prodigious writing." What he was writing about appears not to have been very important, but it seems to have done the trick. No unpublished collections of papers are evident here from any of the relevant archives, and interviews seem to have consisted of "a Sunday afternoon with Thurman Arnold discussing a wide range of topics." As a result the biographical base, even at the intellectual level the author is justified in choosing to emphasize, is thin, to say the least.

Arnold's intellectual positions from the thirties on, nonetheless, are dealt with sufficiently to illuminate the author's conclusions, which reveal significant problems that historians of the period are finding increasingly interesting. The attraction of committed American democrats to elitist theory and the relation between social science and the traditional practices of American politics are important topics. The New Deal, and men like Arnold, may reflect a crucial moment in a generation whose simplistic values were so well ingrained that they could take social science as satire or salvation without having to worry about the difference. Freedom of that kind was their luxury and we can perhaps envy it; but it was also the source of a strength and responsibility that, as Mr. Kearny makes clear, it would be perilous to ignore.

BARRY D. KARL
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JOHN M. SWOMLEY, JR. *American Empire: The Political Ethics of Twentieth-Century Conquest.* [New York:] Macmillan Company. 1970. Pp. 250. Cloth $6.95, paper $1.95.

JEROME SLATER. *Intervention and Negotiation: The United States and the Dominican Revolution.* Foreword by HANS J. MORGENTHAU. New York: Harper and Row. 1970. Pp. xviii, 254. $7.95.

The *American Empire* is a critique on ethical grounds of United States foreign policy from 1939 to 1969 written by a professor of social ethics trained as a political scientist. As he says, "ethics are relevant to foreign policy," if for no other reason than many in the United States believe that their country's actions must be, and are, based upon fundamental moral principles. Professor Swomley regards American policy as consistently immoral and calls for an ethics of "dynamic social change" as the guide for American actions. Analysis of the morality of foreign relations is very much needed today, but the results here are often simplistic and superficial. There are two chapters on Latin America. In one the Alliance for Progress is dismissed in a sentence although it certainly was an important ethical issue. In the other the Castro revolution is examined in the light of the ethics of great-small power relations.

In happy contrast Professor Slater's *Intervention and Negotiation* seeks to correct the grievous errors of Theodore Draper and others who have written extensively on the Dominican intervention of 1965–66. The conditions under which this book was researched present a problem. Many of Slater's written sources were (and are) classified and his interviews with United States and other officials were not for attribution. As with the "Pentagon papers," we cannot be certain what was not available to the author. The forthcoming publication of Abraham Lowenthal's study of the intervention based upon the same sources will presumably provide a basis for comparison.

The intervention, Slater believes, resulted from the "no second Cuba" doctrine. This "thralldom" that Cuba held over American policy was based upon fear of domestic repercussions in the United States and a cold war mentality that vastly overrated the putative dangers of a second Castro to American interests. Although he concedes a potential for a Communist take-over, Slater argues that it was small. Once the United States had mistakenly decided to intervene, it should have supported the non-Communists (including Juan Bosch) among the revolutionists. It should also have carried through with its original commitment to reform the military. In the early stages of the revolt, Ambassador Bennett relied on the military to block Bosch and the Constitutionalists; later, Washington came to rely on them to support a postrevolutionary democratic government. So the military establishment emerged virtually intact. Yet the United States's desire for reform and democracy

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shifted its attitude toward the Constitutionalists half circle, so that in the 1966 election, John Crimmins, ambassador-designate, worked hard to keep Bosch in the race. The United States successfully insisted on free elections and was prepared to support Bosch, if elected. OAS and UN organizations were "peripheral" throughout, except for the Inter-American Peace Force, which the United States could control, if sometimes with difficulty. Slater has deservedly high praise for Crimmins and Ellsworth Bunker. His disdain for John Bartlow Martin, Bennett, and the latter's deputy, Connett, seems not to rest on first-hand knowledge.

The reader must always consider carefully the conditional words used by Slater; for "presumably," "perhaps," and "probably" are very much in order when the record is only partially available. One can assume without denigrating the value of this volume that much will be corrected when the documents are open. The author conveys the complexity of the pressures that limited and shaped United States policy, and he marshals carefully his evidence on the alternatives available to the United States. He has made a major addition to informed discussion of United States foreign policy.

DAVID D. BURKS
Hunter College

DILLON S. MYER. *Uprooted Americans: The Japanese Americans and the War Relocation Authority during World War II.* Tucson: University of Arizona Press. 1971. Pp. xxx, 360. $8.50.

AUDRIE GIRDNER and ANNE LOFTIS. *The Great Betrayal: The Evacuation of the Japanese-Americans during World War II.* [New York:] Macmillan Company. 1969. Pp. x, 562. $12.50.

Dillon Myer was the director of the War Relocation Authority (WRA), the civilian agency that took charge of relocating one hundred ten thousand persons of Japanese ancestry, alien and citizen alike, after President Roosevelt by executive order had given carte blanche to the army to evacuate them from the West Coast in February 1942.

Myer admits in chapter 1: "Neither I nor most of my staff were well informed regarding the problems we faced. We lacked information about the evacuees and their history. We were generally uninformed regarding the anti-Oriental movements on the West Coast, the pressures, rumors and fears that led to the evacuation" (p. 6). From that honest beginning he proceeds to a candid, well-organized, and effectively told story of the relocation and its people, down to the closing of the last relocation center (Tule Lake, California), and the return of the last of the evacuees to the West Coast or (in the case of "renunciant" repatriates) to Japan in 1946.

It is the director's story, mainly derived from his own reminiscences and documents to which he was a party, but, as attested by the Japanese American Citizens League (foreword), he was a humane and understanding director who did his best for his charges, the evacuees. He made the critical decision, and fought for its implementation, that the relocation centers were no proper place for people to stay, and much of the story is concerned with the efforts of his WRA administration to move them out of the centers to whatever job or educational opportunities could be created in middle or eastern America, and ultimately back to the West Coast. He takes some justifiable pride in the fact that most of the relocation centers closed ahead of schedule (p. 211). Even after the centers were closed the WRA did not abandon the evacuees, but created a hostel system to provide shelter in the tight-housing situation of 1945–46, especially in California, and it assisted them with property restitution problems.

One of the most interesting chapters is that entitled "Summary of WRA Relations with Other Governmental Agencies" (ch. 18). Myer pulls no punches in sorting these out. He names names and divisions. Except for the Quartermaster Corps, the War Department—including Secretary Stimson—was a source of frustration in all the decent things Myer tried to do, as was the FBI and all Western governors, except Carr of Colorado. The Federal Reserve Bank did a deplorable job with evacuees' property (ch. 19).

The Great Betrayal adds compelling and colorful detail to the story. Its 482 pages of text are replete with incidents, names (a few misspelled), and quotations. Some of these, not only from the Hearst press and the American Legion, but from government officials, are simply appalling in retrospect. Utilizing private papers, diaries, and interviews as well as gov-

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ernment records, the authors' accent is on the personal and the immediate experiences of the evacuees and those who tried to help them. Dillon Myer is evaluated highly, as are WRA administrators generally. "But for them, a large number of evacuees would have become completely disenchanted with American life" (p. 240). Before they came on the scene, however, much damage had been done at the race track assembly centers in California. In particular, "the Nisei were not prepared to believe that their citizenship would count for so little" (p. 32). The case of Grace Fujii, who, twenty-four hours after taking her comprehensive examinations at Mills College and winning a diploma and a Phi Beta Kappa key, "was taken off to a horse stall at Tanforan" (p. 244) illustrates the irony and the bitterness of many situations.

Both books end on a note of optimism: "Progress Among the Nisei" (Myer) and "Democracy Corrects Its Own Mistakes" (Girdner and Loftis). Twenty-five years later there is almost no overt prejudice against Japanese Americans and over five hundred discriminatory laws have been repealed. The story of the evacuation "will stand as an aberration and a warning" (Girdner and Loftis, p. 482).

Is that all, or is it a rehearsal for 1984, with primary targets other than Japanese Americans? Mentioned, but not sufficiently stressed by either study, is the problem of the "renunciants," those who refused to cooperate with WRA or to swear loyalty under stress and were sent off to Tule Lake as recalcitrant "extremists." Sociologists Dorothy S. Thomas and Richard Nishimoto have discussed these in *The Spoilage* (1946), but not in the perspective of twenty-five years later. They are a troublesome minority but not the central issue in both books under review. So further study is needed; how does American democracy treat those who refuse to be "good citizens" and accept the "temporary" violation, not only of their constitutional rights, but of their inalienable rights as human beings? Neither study quite comes to grips with this larger aspect of the problem, though each is important, useful, and excellent in its own way.

HILARY CONROY
University of Pennsylvania

Foreign Relations of the United States, 1946. Volume 2, *Council of Foreign Ministers;* Volume 3, *Paris Peace Conference: Proceedings;* Volume 4, *Paris Peace Conference: Documents.* (Department of State Publications 8497, 8491, and 8492.) Washington: Government Printing Office. 1970. Pp. xiii, 1586; xl, 882; viii, 956. $7.50; $5.25; $5.50.

The editors of *Foreign Relations* under the superb supervision of S. Everett Gleason have herein given historians a huge reminder—3,424 pages in three volumes—that after the Second World War the victorious Allies in 1946 managed to draw up peace treaties for five European defeated nations (Italy, Bulgaria, Hungary, Romania, Finland) and establish the Free Territory of Trieste. Merely to detail the meetings and recommendations of the Paris and New York sessions of the Council of Foreign Ministers and of the twenty-one Allied nations, large and small, assembled in the Paris Peace Conference of July–October is to show the necessary size of the documentation. Beginning in January 1946 the CFM's deputies held over fifty meetings in London and Paris, and there followed a session of the CFM that ran to forty-two formal and fifteen informal meetings. The Paris Peace Conference considered the resulting draft treaties and made fifty-three recommendations by at least a two-thirds majority vote, and forty-one by majority vote of less than two-thirds. A session of the CFM in New York adopted forty-seven of the former and twenty-four of the latter. The treaties were signed on February 10, 1947.

One must say that the Historical Office of the Department of State again has done a magnificent job of organizing this massive documentation. The increasing complexity of the Department's files is evident in the fact that the numerical central files were only a part of the material that the compilers had to use. There also was Lot M–88, one of the now famous "lots" of documentation apart from regular files, which contained the CFM file. There were "unconsolidated files" within the Department. Compilers had to investigate the commentaries in memoirs by Secretary of State James F. Byrnes, and *The Private Papers of Senator Vandenberg* (1952) edited by the son of the late Senator. The office has composed introductions to all three volumes relating the na-

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ture of the documentation. One volume has a long listing of individuals mentioned, together with their titles. Everything is carefully indexed.

Volume 2 sets out the preparations for and then the actual meetings of the CFM in Paris running intermittently from April to July, and the meetings in New York in November–December. Volumes 3 and 4 are exclusively on the Paris Peace Conference, with volume 3 containing the proceedings and volume 4 the draft treaties, diplomatic correspondence, and miscellaneous documentation. Proceedings of the conference were public, and most of the documents were unclassified. Some of them appeared in two contemporary compilations, one by the Department of State; but both of these compilations had a limited distribution, and the Historical Office properly has repeated the important documents. A good deal of the information on the proceedings, incidentally, is from the United States delegation's journal, a running summary. There appears to have been a lack of high-level documentation. As the editors explain: "Although Secretary Byrnes often met with various subordinates during morning hours, no records of these meetings are known to exist. The United States Delegation neither held formal meetings nor, aside from the dispatch of segments of the Journal at irregular intervals, reported to Washington in a systematic way."

What can one make of all this material? If it has the appearance of tedium, that appearance vanishes once one begins to read, for there are fascinating bits and pieces here. "Imagine, fellow delegates, a long, long peace, maybe even permanent peace—wouldn't that be wonderful!" So said Jan Masaryk in August 1946 only months before his tragic and still somewhat mysterious death. Molotov's heavy jokes appear, as when he told Ernest Bevin that "Mr. Bevin was always pleased when he saw that he was likely to obtain something." At another point, after Secretary Byrnes had indulged in an historical excursion concerning the Yugoslav frontiers, the Russian foreign minister told Byrnes that Napoleon had considered Trieste, Gorizia, and Gradisca to be Slav "although it is known that Napoleon was not a Yugoslav himself."

But there is far more than bits and pieces here; there is an almost photographic account of "what really happened at Paris," to use Charles Seymour's title for 1919. No one can read this material and deny that there was Russian intransigence after 1945, if not before. These three volumes underline the fact that the present-day argument of the self-styled revisionists over the origins of the cold war has been conducted largely without benefit of the sources, and that the revisionists had better call a moratorium on their contentions while they repair to the new volumes of *Foreign Relations* and thence to the Department of State archives, now open, where they will find, quite literally, lots of information.

ROBERT H. FERRELL
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G. BERNARD NOBLE. *Christian A. Herter.* (The American Secretaries of State and Their Diplomacy, Volume 18.) New York: Cooper Square Publishers. 1970. Pp. xii, 333. $7.95.

Christian A. Herter was Eastern establishment all the way—years of residence in Europe, private schools, Harvard, the Foreign Service in 1916, marriage to Mary Caroline Pratt of Pratt Institute heritage a year later, and membership on the American Peace Commission in 1919. This was a promising start for a man scarcely twenty-four. Still, Herter left public service in 1924 to assume an editorship of *The Independent.* Later he turned to Massachusetts state politics with considerable success and in 1942 won a seat in the United States House of Representatives by a narrow margin. After ten years in the House he ran successfully for the governorship of Massachusetts in 1952 and 1954. Finally, in February 1957, he entered the Eisenhower administration as undersecretary of state, handling duties of little concern to Secretary Dulles. With the latter's terminal illness in the spring of 1959, Herter was appointed secretary of state. In his first assignment he disposed of the immediate Russian challenge to Berlin and Germany at the Geneva Conference of May–August 1959.

Herter obviously was not appointed to redirect the foreign policies of the United States. Indeed, he seems never to have questioned his inheritances. Yet in 1959 the Dulles policies toward Eastern Europe, Germany, China, Indochina, and even Latin America were hardly sol-

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vent. Nor would Herter triumph where Dulles had failed. Somehow almost every external challenge facing the United States—Vietnam, Laos, Cuba, the Middle East, Africa, China, and the USSR—was more troublesome in 1961 than it had been two years earlier. Despite Noble's strenuous efforts at approbation, this volume makes clear why Herter's reputation as secretary of state is not destined to improve with the passage of time. Herter possessed many excellent human qualities, but he was in no way uniquely qualified for the office of secretary of state.

If Herter as secretary revealed little critical judgment, what about the author? That Herter perpetuated objectives that transcended the means available for their achievement has escaped Noble completely. Herter refused, for example, to analyze SEATO or the Eisenhower policy that anticipated nothing less than a victory for South Vietnam without any direct American military involvement. The historian should certainly be able to detect such fallacies and explain why resultant policies are destined to lead either nowhere or into trouble. Unfortunately the author no more than the secretary could transcend the official assumptions and assurances of success. Still one would hope that the historian might do better.

NORMAN A. GRAEBNER
University of Virginia

ERIC ROSS. *Beyond the River and the Bay: Some Observations on the State of the Canadian Northwest in 1811, with a View to Providing the Intending Settler with an Intimate Knowledge of That Country.* [Toronto:] University of Toronto Press. 1970. Pp. xiii, 190. $8.50.

Beyond the St. Lawrence River and Hudson Bay—well beyond in the case of the river—lay the great Northwest, the scene in 1811 of increasingly bitter fur-trading rivalry between the Montreal-based North West Company and the Hudson's Bay Company. Eric Ross, a historical geographer, has invented an imaginary author of the period to give a detailed and vivid account of the Northwest as it then was, an account based on the vast range of information to be found in journals, diaries, letters, and other sources extant at the time. He does for the Canadian Northwest in 1811 what Ralph H. Brown did back in 1943 in his *Mirror of the Americas* for the eastern seaboard of the United States in 1810.

A geographer who has done the thorough historical research displayed in the text of this volume as well as in its excellent bibliography is especially well qualified for the task Ross has accomplished. An understanding of the geography of a region is always important for an understanding of its history. It is particularly important, however, when, as in the case of the Northwest in 1811, the region is vast and primitive, is subject to great climatic variations, contains plains and mountains, grasslands, forest, and tundra, and derives its economic importance from a fur trade dependent on lengthy and difficult lines of communication and on the services of varied tribes of Indians and Eskimos all close to nature.

Finally, and adding to the usefulness of this volume as an introduction to the Northwest, Ross has an eye for significant and interesting detail about the natives and the traders, their dwellings, food, and clothing, their endurance and ingenuity in overcoming physical difficulties, and the rapidly changing economy that they were at the same time creating and destroying.

D. G. G. KERR
University of Western Ontario

C. P. STACEY. *Arms, Men and Governments: The War Policies of Canada, 1939–1945.* (Published by authority of the Minister of National Defence.) [Ottawa: The Queen's Printer for Canada.] 1970. Pp. xi, 681. $12.00.

Before 1939 Canada's military potential was practically non-existent, her military leaders had no voice in defense policy and were held in little esteem by the government, and her political leaders and top civil servants were inclined to pacifist and isolationist policies. Six years later the Canadian war effort in Europe and the Atlantic was proportionate to the country's wealth and population, which means that it was out-classed only by the great powers. In recounting the story of this transfiguration Colonel Stacey has prepared a blueprint for national organization for war that is more useful because Canada's exclusion from participation in the making of allied strategy means that he can deal with administration and relations with allies without being diverted by op-

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erations. The book therefore serves more effectively the purpose for which general staff histories are written, to record experience for future use.

This official history also breaks new ground by an analysis of Canadian relations with Britain and the United States during the war. The author starts from the premise that it is to the national advantage of a small power to keep all its forces together under the command of one of its own leaders. He shows that the R.C.N.'s role in Atlantic convoy duties achieved this end incidentally (but with operational control by the United States even when the American contribution was negligible), that the army was concentrated only at the end of the war, and that the R.C.A.F. never obtained more than partial operational identity. For these shortcomings, Stacey blames the government's parsimony before the fall of France, Prime Minister Mackenzie King's desire to make the Canadian war effort solely through industrial production and the Commonwealth Air Training Scheme, the desire of some Canadian soldiers and some members of External Affairs to get Canadian troops into action even at the cost of splitting the effort between two theaters, the attitude of some British generals toward "colonial" troops, the obstruction of R.A.F. training wings and of young fliers enthusiastic for mixed crews, the failure of Canadian ministers visiting London to speak their minds, and the rigid nationalism of American military leaders. As a consequence, Canada did not get an adequate voice in strategy making, and often not even the courtesy of being informed how her troops were to be used. The book also includes an account of Canada's conscription for home service only. This has lessons applicable in the United States today.

Although belated appearance is not solely due to the book's treatment of political questions and of Mackenzie King, there is no doubt that the delay has made it possible for this official history to give a fuller and truer view. Fully documented, packed with detailed information, this fine volume is a masterpiece of readability, a fitting conclusion to the set of five volumes that Stacey's Canadian Army History section produced on World War II.

RICHARD A. PRESTON
Duke University

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Communications

A communication will be considered only if it relates to an article or review published in this journal; publication of such a communication or of any part of it is solely at the editors' discretion. Limitations of space require that a communication concerning a review be no longer than the review to which it refers and in no case longer than 500 words. Communications concerning articles or review articles may be no more than 1,000 words, and the editors reserve the right to impose a lower limit. The schedule of publication and the time needed to send a communication to the author of the article or review in question for such reply as he may care to make virtually preclude the possibility of publication in the issue following that in which the original article or review appeared. Unless, in the editors' judgment, some major scholarly purpose is served, rejoinders will not be published.

TO THE EDITOR:

I do not wish to cavil with the judgments expressed by Paul J. Pinckney in his comments on my book, *The Puritan Lectureships* (*AHR*, 76 [1971]: 145–47), but I would like the opportunity to correct a factual error contained in his review. On page 146 he writes: "After showing his distaste for the Caroline regime, Seaver says near the end that 'there were more Puritan lecturers in the City in 1640 than in any year before 1626.' But Seaver's tables show that he should have said 'fewer,' not 'more.' How is one to explain this slip of the pen?"

Professor Pinckney has quoted me quite correctly, and a glance at my table 8 (p. 245) will show that I had found forty-two Puritans lecturing in London in 1640. Table 7 (p. 203) shows forty-three Puritan lecturers in London in 1626 but only thirty-seven in 1625, and in fact, if my figures are correct, thirty-seven was a figure that had only been reached once before (in 1623). Hence, since forty-two is a greater number than thirty-seven, it does seem fair to assert, as I did (p. 266), that "there were more Puritan lecturers in the City in 1640 than in any year before 1626." Hence, too, there is no need to explain the nonexistent slip by implying that my "commitment to the overthrow of the old regime" rendered me unable to read my own tables.

PAUL S. SEAVER
Stanford University

PROFESSOR PINCKNEY REPLIES:

My public apologies to Mr. Seaver, whom I met recently in London and found to be kind, generous, and brilliant. The best rationalization that I can muster is that the placing of his true statement in the final paragraph of the chapter on Laud led me (perhaps logically but for an instant incorrectly) to think of the Laudian years under discussion and somehow to turn "before 1626" into "since 1626" in my mind. Thus neither of us misread his tables—I simply misread his statement. At least ten times! In any case, my slip concluded what I meant to be a very favorable review.

PAUL J. PINCKNEY
University of Tennessee

TO THE EDITOR:

This is a letter that I was sure someone else would have written by now. In view of the critical response to Earl J. McGrath's *The Pre-*

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dominantly Negro College in Transition (New York, 1965) and Christopher Jencks's and David Riesman's "The American Negro College," *Harvard Educational Review*, 37 (1967): 3–60, one has to admire Professor James McPherson, another white "liberal," for bringing forth his "White Liberals and Black Power in Negro Education, 1865–1915," *AHR*, 75 (1969–70): 1357–86. Yet because, in large part, it contains some of the same basic defects as the previously mentioned works, I have been awaiting the flood of critical replies that greeted those studies. Thus far, however, the only reply to Professor McPherson's article has been the letter of Messrs. Murray and David Levine of New York (*AHR*, 76 [1971]: 581–82), who were mainly interested in the modes of statistical interpretation.

Among the basic defects in McPherson's article, I will mention two here. The first, and most serious, is his overestimation of the financial support that whites have given to black education and his underestimation of the sums given by blacks for the support of their own schools (here he follows a tradition typified by Ullin Leavell in *Philanthropy in Negro Education* [Nashville, 1930]). Indeed, this very important matter is merely glossed over. One should expect that McPherson would have cited the annual contributions blacks began to make toward their own education after they became able to do so. Horace Bumstead, one of the early white presidents of Atlanta University, placed a question in 1891 that McPherson should have surely treated: "Where in the history of the world has so large a mass of equally poor and unlettered people done so much to help themselves in educational work?" The students of Atlanta University were then paying thirty-four per cent of the expenses of that institution, about the same proportion paid by students at Harvard. Numerous observers of black education in the South have praised the sacrifices that blacks have made to sustain education, even though their resources have been most meager. (See, for example, Samuel Barrows, "What the Southern Negro is Doing for Himself," *Atlantic Monthly*, June 1891, pp. 810–12 as well as my own edited work, *In the Cage: Eyewitness Accounts of the Freed Negro in Southern Society, 1877–1929* [Chicago, 1971]).

The second point to be made here is that Professor McPherson appears to make attendance at certain black colleges responsible for the later success of such men as W. E. B. Du Bois, James Weldon Johnson, Martin Luther King, Jr., and Stokely Carmichael. Even though such relations are often made, I do not know that one can produce scientific proof that attendance at a particular college at a particular time is the major correlation to later success; at least Professor McPherson offers no such proof. Professor McPherson further takes it upon himself to identify the best of the black colleges, first in 1915 and then currently. In one of his expansive footnotes, after citing a favorite source, Thomas Jesse Jones's monumental *Negro Education* (1916), McPherson calls Morehouse and Wiley two of the best colleges in 1915. He cites no criteria for that judgment. Thomas Jesse Jones, on the other hand, remarked in 1916 that "hardly a colored college meets the standards set by the Carnegie Foundation and the North Central Association." He called Fisk and Howard "colleges," but designated Morehouse and Wiley as "secondary and college," which according to the Jones study would place them below Fisk and Howard. Similarly, Professor McPherson gives no criteria for adjudging such schools as Fisk, Lincoln, Morehouse, and Spelman as the best black colleges today. One suspects that the application of certain standard criteria would bear him out, but evidence, rather than speculation, would be more appropriate for a scholarly study. Such evidence is particularly appropriate, and this is one of the telling criticisms of such works by white "liberals," considering the fact that the author has probably never attended a black college or taught at one for any measurable length of time.

ALTON HORNSBY, JR.
Morehouse College

PROFESSOR MCPHERSON REPLIES:

I shall try to respond to Professor Hornsby's three criticisms in the same order as he presented them.

Perhaps I did not devote sufficient attention to the question of black financial support for the mission schools, but I do not think I can fairly be charged with overestimating the amount of white support. I quoted Adam Clayton Powell,

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Sr.'s statement in 1930 that blacks had paid only ten per cent of the cost of their higher education since emancipation and commented that Powell had probably underestimated the black contribution (p. 1377 and n. 54). In treating the growth of black power in Methodist colleges, I noted that such growth corresponded with an increase in black financial support of these schools from about fifteen per cent in the 1890s to thirty-three per cent by 1916 (exclusive of tuition and fees) and that if tuition, room, and board were included, Negroes had contributed nearly forty per cent of the cost of their education in Methodist schools down to 1908 (pp. 1369–70 and n. 34). The same percentage, incidentally, held true for schools controlled by the Baptist Home Mission Society (*Home Mission Monthly,* 19 [July 1897]: 260). As I noted in note 56 the degree of black power in Methodist and Baptist schools was greater than in those of the Congregational American Missionary Association because of the larger black membership in the former denominations and the correspondingly larger black financial support of their schools.

I did not state that attendance at certain black colleges was "responsible" for the later success of the prominent black men named. But some of these men came close to stating this in their own memoirs. James Weldon Johnson praised his alma mater, Atlanta University, as "an excellent school. . . . For me there was probably no better school in the United States" (*Along This Way* [New York, 1933], 83–84). Du Bois declared that "the excellent and earnest teaching" he received at Fisk "enabled me to arrange and build my program for freedom and progress among Negroes." In *The Souls of Black Folk* Du Bois praised the crusade for freedmen's education as "that finest thing in American history," which brought "the best of the sons of the freedmen" into "close and sympathetic touch with the best traditions of New England. . . . In educational power it was supreme" (*The Autobiography of W. E. B. Du Bois* [New York, 1968], 112; *The Souls of Black Folk* [New York, 1903], 100).

I am puzzled by Professor Hornsby's challenge to my ranking of black colleges. I have reread my article carefully and nowhere in it can I find any reference to Fisk, Lincoln, Morehouse, and Spelman as "the best black colleges today," though I agree with Professor Hornsby that they may deserve such a description. The closest I came to such an assertion was the statement that Morehouse and Spelman became after 1900 "two of the best Negro Colleges in the South" (p. 1373). As for my inclusion of Morehouse and Wiley "among the best Negro colleges" in 1915, such a judgment is relatively noncontroversial and widely agreed upon. See especially W. E. B. Du Bois, *The College-Bred Negro American* (Atlanta, 1910), 22, which ranked Morehouse and Wiley among the seven best colleges (ahead of Howard, incidentally). Explicit as well as implicit throughout my article is the assumption that Fisk, Howard, and Atlanta were the best black colleges at that time, but Wiley and especially Morehouse were not far behind. I cited these two schools in order to show that a predominantly black faculty did not preclude quality education, a position with which I hope Professor Hornsby would agree.

JAMES M. MCPHERSON
Princeton University

TO THE EDITOR:

Professor William G. McLoughlin in his review of Martin E. Marty, *Righteous Empire: The Protestant Experience in America* (*AHR,* 76 [1971]: 829–30) makes a serious factual error—not found in the Marty volume—when he states, "Only Lincoln, a non-churchgoer, rises above the carnage." Lincoln, like many others of his contemporaries, was not a church member, but all competent Lincoln biographers, not to mention the religious enthusiasts, clearly indicate that he was a regular churchgoer. After about 1850 he regularly attended the First Presbyterian Church, Springfield, Illinois, where he was the personal friend of the pastor, the Reverend Dr. James Smith. After becoming president he took a pew in the New York Avenue Presbyterian Church in Washington and became a friend of the minister, the Reverend Dr. Phineas D. Gurley. See, for example, James G. Randall and Richard N. Current, *Lincoln: The President: Last Full Measure* (New York, 1954), 373; and Benjamin P. Thomas, *Lincoln* (New York, 1952), 478. One also might cite numerous

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contemporary accounts, such as Bascom N. Timmons, *Portrait of an American* (New York, 1953), 9.

FRANCIS P. WEISENBURGER
Ohio State University

PROFESSOR MCLOUGHLIN REPLIES:

Professor Weisenburger is right. I should have said "non–church member."

WILLIAM G. MCLOUGHLIN
Brown University

*The following letter is in reply to a communication from David S. Landes (*AHR, *76 [1971]: 1633–37) concerning the review article by R. M. Hartwell and Robert Higgs, "Good Old Economic History"* (AHR, *76 [1971]: 467–74), on Mr. Landes's* The Unbound Prometheus.

TO THE EDITOR:

We are surprised by the ferocity of Professor David Landes's reply to our review article. Although we criticized the book on several grounds we also said that "anyone interested in the industrialization and economic growth of modern Europe on a comparative basis can best start with Landes's book, which is unique in its coverage and impeccable in its historical scholarship. It is impossible not to admire the vast range of facts that Landes commands and the way he has organized them into a readable and purposeful account of European industrialization. Certainly this book will long remain the historians' textbook on the economic growth of modern Europe" (p. 470). In reply to our praise and criticism of "a magnificent piece of old economic history," Landes claims that our review has "an aura of esoteric competence and specialized terminology," expressed in "elliptical phrases"; that it has "sloppiness" and is "incompetent"; that it contains a "debris of error," "nonsense," "misleading and erroneous" pictures, and "ludicrous" reproaches; that it is "slipshod nitpicking." Not content with our review, however, he goes on to attack us personally and professionally, claiming that we "neither write nor count well," describing us as "sun-worshippers" who "lose touch with reality" and proclaim a "new orthodoxy" with "arrogant contempt" for traditional history. Finally he diagnoses our ailments as the result of a "small but inadequate dose of economic theory." Hartwell and Higgs, Landes cautions the historical profession, are "not economists" but rather "the camp followers of economics, trying to earn themselves some kind of membership by being more papist than the pope." This is not the language of scholarly controversy; it is personal abuse and *ad hominem* argument that is neither dignified nor convincing. We deserved some reproach for a piece of rudeness in our own review, for which we now apologize: the implication, put as a question, that Landes "is ignorant of?" the economists' literature on technological change. This was an honestly made inference from the text and documentation, rather than "a gratuitous swipe," and any offense we gave has certainly been repaid with ample insult. It is true, also, that we failed to notice that Landes did refer to a work by Edwin Mansfield but, we insist, this does not invalidate our observation that generally he ignores such literature. For example, although the book is impressively documented (one of its virtues) we find only seven unambiguous references to economists on technology in the first six chapters (and none in chapters 1 to 4); even in chapter 7 the only significant use of economists seems to be on pages 518–19, where three sources are cited. Certainly, as Landes claims, he makes more use of economists in chapter 7 but his figures of citation are misleading in that most of the economists he refers to, relevant as they are for other themes, are not concerned primarily with technological change. And surely it is interesting but irrelevant that "first-rate economists" have read his manuscript; Landes alone is responsible for his writing, and what we are challenging basically is not the documentation, which is excellent, and not his advisers, who were certainly first-rate, but the content of his analysis as published in his book.

Landes quarrels with us on two levels: first, on a variety of small points, mainly involving alleged carelessness and triviality ("nitpicking"), and second, on substantial issues of theory. A point-to-point review of the small points would be tedious and lengthy. Anyone interested in these details can satisfy himself as to who is in error by checking the pages cited in our review.

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(A careful checking shows that there are sixty-four page references to Landes's book in our review, a selection from a much longer list; one on page 472 has been incorrectly printed [a "359" should be a "539"] on page 473 we should have cited pages "190–91" instead of page "191"; we misplaced two citations—"520" on page 471 and "474" on page 473 illustrate points different from those given in the text. In citation of other authorities we do not reproach Landes for not having read books not published or printed after the appearance of his book, as he alleges, but use them only to make methodological or theoretical points.) Sometimes our disagreement is partly confusion. For example, Landes criticizes us for criticizing him for having only three tables on "early industrialism." The statistics about Landes's tables are as follows: of a total of fifty-seven tables, forty-one refer mainly to the period after 1918, eight mainly to the period 1870 to 1914, five mainly to the period 1850 to 1870, and three mainly to the period before 1850; only two have any statistics for the period before 1815 and only seven refer to the period before 1850; there are sixteen tables for the period before 1914 (not fifteen, as Landes strangely asserts). We still think this weighting unbalanced. Certainly the tables can be interpreted in various ways, and it is obvious that Hartwell and Higgs have different ideas from Landes both about the dating of "early industrialization" and also about the desirability of using tables to illustrate that industrialization. Turning to more substantial differences between Landes and us, let us first agree that a major criticism of ours was that Landes did not write the sort of book that we think he should have written. He wrote what we have called "good old economic history," a phrase used not "scornfully" or "sarcastically" but as praise with, we thought, a touch of humor; we believe, however, that the nature of the subject matter covered by Landes demanded "good new economic history"; we know of a large and relevant economic literature on technological and on productivity change that is relevant for such a study. But lest the reader believe that we are incompetent, and motivated by some strange malice, it is important to note that the appearance of our review coincided with that of two other long reviews: Rondo Cameron's in *Explorations in Economic History,* 8 (1970–71): 229–37, and Nathan Rosenberg's in the *Journal of Economic History,* 31 (1971): 497–500. The substantial similarities between our review and the other two are remarkable. Although these similarities resolve nothing—after all, the four of us could be in clandestine conspiracy against Landes, or we could all be incompetent and careless—they may allay some doubt in the minds of historians tempted to take seriously Landes's attacks on our competence and characters.

Of the more substantial differences between Landes and us, three deserve an additional word.

First, Landes reaffirms his belief that the growth sequence of modern times is that of technological change, industrial development, and economic growth—now with due allowance for feedback—and he asserts that this conception is "acceptable to the vast majority of economists." Neither Landes nor we have any way of knowing what all the world's economists believe about this matter, but the issue is hardly to be resolved by taking a poll. This view of the economic growth process is seriously lacking on at least two counts: First, it ignores agricultural development entirely, which will hardly do in the analysis of economies where agriculture was not only the largest source of employment but also an activity characterized by its own peculiar sources of rising productivity—better crop rotations, selective livestock breeding, drainage and consolidation of fields, and so forth. (For Europe, in general, population remained essentially rural on into the second half of the nineteenth century [in Germany, for example, over sixty per cent of the population was still rural in 1870; in Eastern Europe, the percentage was higher]. In much of Europe, then, there was "a demographic explosion with little more than agriculture to take care of the increase." See Folke Dovring, "The Transformation of European Agriculture," in *The Cambridge Economic History of Europe,* 6, pt. 2, ed. H. J. Habakkuk and M. M. Postan [Cambridge, 1965].) Second, it pays no attention to the establishment of the political and institutional framework of secure private property rights that is a prerequisite if in a market economy many men are to forego consumption and to bear risk by investing in material and human capital and in the search for more productive technical knowledge. (See, for example, Harold Demsetz,

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"Towards a Theory of Property Rights," *American Economic Review,* 57 [1967]: 347–73.) Landes's book cannot be accused of ignoring these subjects; but neither does it devote to them that minimum of attention we expect in a 555-page treatise on economic growth.

Second, Landes says: "I do believe, as do all the economists I read and talk to, that changes in relative factor costs (an increase in the wage rate as against the cost of capital) do induce manufacturers to substitute capital for labor *and* that this substitution almost invariably entails technological progress" (Landes's italics). This is simply false, and an army of economists to the contrary would not make it true. It may be that the economists Landes reads and talks to are not a representative sample; oddly enough, the economists we read and talk to are unanimous in recognizing that factor substitution need not and usually does not entail technological progress, even though it does from time to time with important effects. Factor substitution without any change in the economy's stock of productive knowledge is so familiar a feature of the operation of any market economy that we can only appeal to Landes to open his eyes and broaden his reading.

Third, Landes remains committed to a methodology that seeks to "seize reality in all its complexity." He now admits that certain simplifications may be "an avenue to understanding." But he does not seem to appreciate that the use of economic theory involves more than mere simplification. Let one thing be clear: the economist's methodological approach—which requires him to specify explicitly the theory from which he derives his hypotheses and then to subject his hypotheses to rigorous empirical tests—is not unique to economics; it is the property of scientific inquiry in general. The choice is not, as Landes seems to believe, between simple knowledge and complex knowledge, the two being acquired in quite different ways. Any understanding at all depends on the interpretation of observed events in the light of some theory. If no theory is explicitly specified, then causal statements rest on implicit theory. This is not necessarily a source of error, but it often is, because logical inconsistencies are more difficult to identify when the theory is kept under the table. These points are elementary in the philosophy of science; they have been discussed at great length in the journals of economic history for more than a decade, and increasingly historians of all specialties are recognizing their applicability to their research. Landes remains steadfast in his resistance. Because he does not appreciate the necessity of applying economic theory in gaining knowledge of the economic past, Landes sometimes fails to exploit the explanatory power of elementary theory; at other times his misuse of an implicit economic theory leads him into errors, some of which we indicated in our review (p. 473).

Landes's *The Unbound Prometheus* is an admirable, and in some ways a magnificent work of historical scholarship, but that does not give it immunity from criticism. It is not perfect, and most of its shortcomings are common to all interpretive works that fail to exploit the power of scientific methodology. For us, the most important question raised by Landes's book remains: can economic history be satisfactorily written in isolation from economic theory? Our answer, again, is that it cannot.

R. M. HARTWELL
University of Virginia
ROBERT HIGGS
Nuffield College, Oxford

TO THE EDITOR:

The efforts of psychoanalytic historians must be welcomed as a further step toward deepening our understanding of human motivation. Before such work leads to a meaningful dialogue, however, serious problems of communication must be overcome. I will leave it to the psychoanalytic school to determine what nonexpert historians (grouped under the epithet "the common-sense school") can contribute to this intellectual exchange. For my part, I want to make some demands of the psychoanalysts, taking Peter Loewenberg's "The Unsuccessful Adolescence of Heinrich Himmler" (*AHR,* 76 [1971]: 612–41) as a case in point.

I ask that psychoanalysts approach human personality as something that is, however complex, ultimately coherent and intelligible. Loewenberg's failure to do this makes his study of Himmler sadly incomplete.

Loewenberg's Himmler is "a rigid, repressed character who experienced only weak and limited feelings" (p. 615), a "dull, restricted per-

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sonality" (p. 634). "His emotional life was barren and impoverished, and his expressions were artificial, lacking real relationships" (p. 617). Yet this same man is described as constantly faced with "a desperate struggle for control of his impulses, of his surroundings, of time and money, of words, and of anger" (p. 623). The question is, if Himmler had such weak feelings, why did he have to struggle so to suppress them? I say suppress, not repress, because the diary shows that Himmler was very conscious of his psychological dilemma. In a revealing sentence Loewenberg admits, "He could not endure any anger or feeling" (p. 629). Why not? In one case, at least, this was due to his "feared inner danger of yielding to irresistible impulses" (p. 635). This is Loewenberg's common sense speaking, not his psychoanalysis. Throughout the article he is forced to describe his supposedly emotionless subject in terms of his feelings (see especially p. 622). Such a confusion of language inevitably stems from muddled thought. Loewenberg must either change his vocabulary or his characterization.

The same inconsistency arises when Loewenberg quotes from the diary. There Himmler describes himself as "melancholic" (p. 630) and full of "gloomy thoughts" (p. 620). He is searching for "peace in work" (p. 623). Only in the third person can he admit that "one notices how one thirsts for love" (p. 634). He even professes to have a spiritual nature: "I will always love God, pray to him, and cleave to and defend the Catholic Church" (p. 634). Loewenberg's treatment of such passages is most disappointing. He simply disregards them, denying by implication that Himmler might have had any insights into his own personality worthy of respect. Instead Loewenberg reverts to familiar devices for turning black into white and vice versa. They are reaction formations and projections. The reality of the feelings is further denied. "His expressions of feeling were transient identifications and imitations rather than genuine emotions. They had the rubber-stamp quality of one who sees and feels what he is expected to see and feel" (p. 617). Does Loewenberg really believe he can distinguish between real and unreal feelings? Philosophically speaking, can there be an unreal (nonexistent) feeling? Is this not rather a value judgment than an empirically observable fact? I suggest that these passages have great interest whether deeply felt or not. Surely it says a great deal of a person that he feels compelled to mimic his peers even in the privacy of his diary. As an afterthought, I wonder how many adolescent diaries Loewenberg has seen that do express the sadism and hatred Himmler supposedly kept bottled up.

I offer these criticisms in good spirit. I hope the author can show me where I misunderstood him. The article was stimulating and often quite plausible. But the overall recreation of Himmler's personality is incomplete because it must deny the validity of Himmler's feelings to be coherent. I suggest that Loewenberg, and psychoanalysts in general, pay more respect to their subjects' self-knowledge. After all, history showed that Himmler had cause to distrust and suppress his feelings or the lack of them.

STEVEN J. NOVAK
University of California, Berkeley

TO THE EDITOR:

Mr. Steven Novak has presented some fundamental questions for history in general and for psychohistory especially. Why, he asks, should "weak" feelings be suppressed? What is the difference between "real" and "unreal" feelings? Can a historian presume to judge which feelings are genuine as contrasted with those that are learned responses in conformity with expectations external to the subject? What in the realm of emotions may we take at face value, and where should we look for hidden motives and content?

All scholars who have studied the Himmler diaries are in agreement that their chief characteristic is mechanical reporting and that the young Himmler filled them with trivia. The most recent historian to do so is Josef Ackermann, who finds the youthful Himmler "pedantic," "infantile," showing "an exaggerated love of order," "banal," "minute," "automatic," and "narrow" (*Heinrich Himmler als Ideologe* [Göttingen, 1970], 23–24, 32–33). The question is, What does all this mean? My interpretation differs from others in that I hold this emotional flatness indicates a defense, an unconscious attempt to escape from painful and threatening

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feelings and tumultuous impulses. The trivia is used to cover over and defend against underlying emotions of a shameful, fearful, angry, or sexual nature.

I agree with Novak that "it says a great deal of a person that he feels compelled to mimic his peers even in the privacy of his diary." This is the very heart of my interpretation of Himmler. Novak thinks "that Himmler was very conscious of his psychological dilemma," and that among other avowals, Himmler's professions of having a spiritual and religious nature prove this. Novak charges that I deny the validity of Himmler's feelings, which he wishes to take at face value without further inquiry in depth. He does not accept the idea that there may be authentic, full, rich, well-rounded, alive feelings as well as impoverished, empty, barren, stereotyped, learned, conventional, pseudofeelings that are in conformity with cultural anticipations and are in fact acquired in a predigested form from the environment. The latter characteristics constitute the "as if" personality that is an empirically observable clinical entity. There is also a considerable scientific literature on the expression of emotions in puberty and adolescence. As indicated in my article, I consider the works of Peter Blos, Anna Freud, Erik Erikson, Harry Guntrip and Helene Deutsch as authoritative. The writings of Edith Jacobson, Irene Josselyn, Gerald H. J. Pearson, and Adelaide Johnson on adolescence are also relevant and excellent. If Novak wishes a counter example to Himmler of a person who was in genuine touch with her feelings during adolescence, I suggest that he read Anne Frank, *The Diary of a Young Girl* (1952).

To some people the functioning of mechanisms of defense such as isolation, projection, and reaction-formation is not so alien and incomprehensible as it appears to be to the common sense of Novak. For example, if two ideas are presented in sequence, or a thought and a feeling conjoin, the inference may be made that they are linked in the mind. Often these associations are consciously denied or avoided. They are isolated or split off. Himmler described events in great detail but he indicated no emotion with the events or ideas. This is a misuse of the thinking processes, which in healthy functioning must also rely on isolation for intellection and reasoning. In the maladaptive case, isolation is used in order to avoid painful affects such as anxiety, guilt, shame, or depression. The use of trite, sterile language and clichés also isolates affects and evades emotional involvement. When Himmler writes about superficial, insignificant, and relatively meaningless events for months on end, he is avoiding something that is subjectively meaningful. In Himmler's case the very profuse repetitiousness of content without amplification or affect is the critical indication of an unconscious defense at work. Many accusations against other persons or groups do not refer to specific incidents of incest, rape, or homosexuality at all. They are projections, and although a projection may be a "familiar device" to Novak, it is not invalid. In fact, it is so common as to be virtually universal. When a person projects, he is taking something from within himself and placing it into another person or group. When Himmler links "degeneration," "homosexuality," and "the Jewish question" (p. 635), this is both a significant series of associations and a projection.

There are objective and available criteria for determining the genuineness of emotions. Historians can make very effective use of these in their research without necessarily having a psychoanalytic education. Their use does, however, require that the historian be in touch with his own feelings and conflicts. The indications for the presence of a defense against emotion are the relative availability, absence, degree, appropriateness, and differentiation of affect and the level of anxiety. In the Himmler diaries, when one is aware that the diary is dry, bland, monotonous, and apathetic; when one has the impression that the writer is uninvolved and detached from what he is reporting; then, the question of defense against unconscious feelings becomes relevant. This is especially important when the absence of affect concerns thoughts and events that ought to be highly charged with emotion. An affect is inappropriate when there is a discordance between emotion and idea. It causes the writing to have a bizarre or forced quality, to be contradictory and repetitious. Similarly, when there is an inappropriate intensity of affect we have a sign that a conscious affect is holding an opposite feeling

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in repression, or, as it may be concisely stated—a reaction-formation is at work. For example, the person may be defending himself against anxiety by a variety of self-assessed "supermoral" qualities, as Himmler consistently did.

An awareness of context is required by the historian if words are to be used as the communicators of thoughts. Novak has a way of splicing his quotations from my article to take them out of context. He directs special attention to my statement that Himmler "could not endure any anger or feeling" (p. 629), which is in a comparative context of how he and his brother reacted to their father. In my text the subsequent sentences contrast brother Gebhardt's behavior, which was to get angry, with Himmler's obsessional defense, which is my focus: "Instead, he [Heinrich Himmler] went on errands. His hostility was contained and held in. He did not feel it or discharge it as his brother did. It is this very lack of genuine anger or rage that makes Himmler's personality so flat and colorless." Having picked out one sentence from page 629, Novak couples it with a line from six pages later in my text (p. 635) that says Himmler "feared [the] inner danger of yielding to irresistible impulses." This line explicitly refers to an occasion three years later in the diary where Himmler links "land reform," "degeneration," "homosexuality," and "the Jewish question." The context, I think, makes a considerable difference in imparting meaning.

Novak has the need to repeatedly affirm the validity of insight into the self. He is concerned that "psychoanalysts approach human personality as something that is, however complex, ultimately coherent and intelligible" and that I "and psychoanalysts in general, pay more respect to [our] subjects' self-knowledge." If all things were as they appear on the surface, there would be no point to historical analysis of any kind and reality would be uninteresting. I agree with Novak that self-knowledge is ultimately a most important form of knowledge—if it is true self-knowledge—by which I mean that it includes an intimate understanding in the self of the ambivalent, the denied, the discordant, and asocial aspects of motivation and personality. This is the vital coherence and intelligibility of the self that psychoanalysis offers. Psychoanalysis as a clinical technique is dedicated to enlarging the scope of self-understanding and integrating previously hidden insight into the personality, making syntonic what was previously distonic, or as Freud said: "Where id was, there shall ego be." I believe it can have the same integrative role for history.

Emotions are also "facts" of history, but some historians have an aversion to acknowledging these data about our subjects. Affect is a more reliable indicator of the true state of mind than formally expressed thoughts. The absence of affect indicates the presence of a defensive structure. Feelings have a history as surely as do material artifacts. This history and its ambiguities are worth perceiving and studying with all of the seriousness that we devote to the other data we encounter in documents as we pursue our research.

PETER LOEWENBERG
University of California, Los Angeles

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Recent Deaths

HAROLD H. DUNHAM of Denver, Colorado, died August 12, 1971, after a brief illness. He was sixty-seven years old. A 1926 graduate of Swarthmore College, Professor Dunham continued his studies at Columbia University where he earned his master's and doctoral degrees. He taught at Brooklyn College from 1931 until 1938 when he became a faculty member of Wagner College on Staten Island. He became a historian for the U. S. Army Transportation Corps in 1944. Professor Dunham came to Denver University in 1946 as an associate professor of social science and later became a professor of history, specializing in Western U. S. history. From 1965 until 1966 he was a Fulbright lecturer at the University of Ceylon. He also acted as a consultant to the U. S. Justice Department with the Indian Claims Commission. He had been periodically elected to the University Senate and served as its president from 1967 to 1968. At the time of his death, he was writing a book that would have culminated more than thirty years of research on the problems of the Spanish and Mexican land grants in the American Southwest. His papers have been deposited in the Western History Department of the Denver Public Library. The libraries of the University of Denver have established the Harold H. Dunham Memorial Book Fund, which is being used to enlarge the Western American history collection.

LAWRENCE HENRY GIPSON, British Empire historian, died September 26, 1971, in Bethlehem, Pennsylvania. Appointed professor of history at Lehigh University in 1924, he spent the following forty-six years of his life writing an incomparable series of fifteen volumes on *The British Empire Before the American Revolution.*

Lawrence Gipson's training, like the history he wrote, was a remarkable blending of New World influences with the Old. He was a Rocky Mountain frontiersman as well as a Rhodes Scholar. He was born in 1880 in Greeley, Colorado; shortly thereafter his family moved to Caldwell, Idaho, where Gipson grew up. Among the pioneer employments of his youth were mining, fighting forest fires, and driving a stagecoach. After receiving his A.B. degree at the University of Idaho in 1903, he was appointed the first Rhodes Scholar from his state and a member of the first delegation from the United States to go to Oxford University. In the center of classical learning at Lincoln College, he discarded his conspicuous Western hat and grew increasingly aware of his provincial outlook. In his debates with Oxford students, many of whom were aspiring to build a Commonwealth of Nations, Gipson turned his attention to a careful study of the British Empire. These early forensic collisions were the beginnings of his distinguished career.

Upon his return to the United States in 1907, Gipson was appointed professor of history at the College of Idaho. After three years of teaching, he resumed graduate study at Yale University, and under the direction of Charles M. Andrews, he wrote his doctoral dissertation, which was published in 1920 as *Jared Ingersoll: A Study in American Loyalism in Relation to British Colonial Government.* This first book was awarded the Justin Winsor Prize.

While still engaged in the research and writing of his dissertation, Gipson was appointed in 1911 professor of history at Wabash College. He subsequently organized a department of history and government and remained its head for six years. He was induced to come to Lehigh University in 1924 with the promise of an oppor-

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tunity to devote himself more fully to the research and writing that had become the main goal of his life. He was the head of the department of history and government at Lehigh until 1946, when he was appointed research professor. From 1952, when he was made research professor emeritus, until his death, he quickened the pace of his work, realizing that his time was running out.

Few historians have conceived history on so vast a scale and composed it in so grand a manner. The first three of the fifteen volumes, published in 1936, described the worldwide British Empire as it existed in the mid-eighteenth century. The core volumes of the series narrate "The Great War for Empire." Later volumes delineate conditions leading to the American War of Independence. The last two volumes are bibliographies of published and manuscript materials, volume 15 appearing in 1970. The different volumes of the series have been widely reviewed over three decades in both England and America. Separately and together they have won numerous prizes, including the Loubat First Prize (1948), the Bancroft Prize (1950), and the Pulitzer Prize (1962).

Lawrence Gipson rarely permitted himself to deviate from his major project. He did so in 1951 to accept the Harmsworth Chair in American History at Oxford. He again interrupted work on the series to contribute a volume to the New American Nation Series, *The Coming of the Revolution, 1763–1775* (1954). Out of loyalty to the local community of Bethlehem he took time to write *The Moravian Indian Mission on the White River* (1938).

Gipson's devotion to research was second only to his larger loyalties to scholarship and the history profession. He served his chosen profession in several capacities on different levels. He was past president of the Conference on British Studies, past president of the Historical Society of Pennsylvania, and member of the Council of the Institute of Early American History and Culture. As one of the founders of the Pennsylvania Historical Association, he served it as president and never tired in promoting its success. The Association commemorated Gipson's achievements by devoting one issue of its quarterly journal, *Pennsylvania History* (January 1969), to an appraisal of his life and work. The Lehigh local chapter of Phi Alpha Theta was founded and faithfully supported by Gipson; even in his eighties, he regularly attended its meetings.

At the time of his death Lawrence Gipson was eagerly engaged in the writing of the history of Lehigh University, and this was a labor of love for the institution that had so long nurtured his scholarship. It was his final goal, and that his life would not be long enough to reach it was his last disappointment.

R. G. COWHERD
Lehigh University

CARTER GOODRICH was born in Plainfield, New Jersey, on May 10, 1897. He died in Mexico City on April 3, 1971, a victim of a traffic accident.

His academic career involved five universities. His baccalaureate degree in economics came from Amherst College in 1918. His doctorate was from the University of Chicago, where his dissertation was supervised by Professor Harry A. Millis. He was on the faculty of the University of Michigan from 1924 until 1931, was professor of economics at Columbia University from 1931 until 1963 (when he technically retired), and was Andrew W. Mellon Professor of History at the University of Pittsburgh from 1963 until his death.

Professor Goodrich's perception of the academic world involved not only teaching and research but considerable public service. He was resident commissioner for the United States in Geneva in the International Labor Organization in 1936–37, and from 1938 through 1946 he was the principal delegate of the American government to the International Labor Organization. He was chairman of that organization's governing body during the critical war years, 1939–45. He was active in the development of the American Social Security System in the mid-thirties. He helped organize the United Nations Scientific Conference on the Conservation and Utilization of Resources in 1948 and 1949 and was the special representative of the secretary general of the United Nations in Bolivia in 1952 and 1953. He also served as the chief of the United Nations Economic Survey Mission to Vietnam in 1955 and 1956.

His early writings were in the field of labor economics. Two of his earliest books, *The Frontier of Control* (1921) and *The Miner's*

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Freedom: A Study of Working Life in the Changing Industry (1925) are studies in the development of the labor movements of two countries. In the 1930s he helped organize and supervise a study at the University of Pennsylvania on migration economic opportunity.

Historians best remember him for his interest in the effect of the frontier on national development and the role of government in economic development. His seminal essay, "The Australian and American Labour Movement" (*Economic Record*, 4 [1928]: 193–208) identified the role of a hard frontier (as in Australia) in the development of the political labor movement. It contains references to the New Zealand experience, where the frontier, as in America, was really quite "soft."

His other work on the frontier of particular interest was a two-part essay that he wrote with a collaborator, Sol Davidson ("The Wage-Earner in the Western Movement," *Political Science Quarterly*, 50, 51 [1935–36]: 161–85, 61–116). This set of articles was an empirical testing of Frederick Jackson Turner's "safety valve" thesis. Goodrich and Davidson, attempting to verify empirically the Turner assertion of the impact of the frontier on the American labor movement, discovered that insofar as the data could be assembled, those who moved to the frontier were not the unemployed workers from large cities, as Turner has suggested; instead, there seemed to be a general westward displacement, with unemployed workers on the East Coast moving slightly inland, those living inland moving slightly further to the west, and so forth, until "the frontier settlers or their children" of a previous decade chose to move to the new frontier. Goodrich's handling of his findings illustrates to a great degree the criteria he used for scholarship. He noted that the Turner thesis as generally understood was not correct, but he also noted that a correction of the thesis was possible. In brief, Goodrich was not of the "intellectual rigor at all costs" persuasion; he had no pride in faulting an erroneous statement in order to bury the reputation of its original formulator. He preferred, rather, to restructure what was correct in order to improve the product.

His course in American economic history at Columbia University was taken by virtually all economics students and a great many historians as well. He was particularly interested in what had been for several generations a running debate: Was the government "the" creative force in economic development, or was it simply an element entering before or after the private sector (presumably the alternative creative force) got into the act? This debate, known to one earlier generation as "Sir William Ashley/George Unwin alternative interpretations," was something Goodrich had considered in his early books. By 1931 he was part of a panel at the annual American Economic Association meetings, where he discussed "private enterprise in economic history." This interest waxed, and after the Second World War he directed an increasing number of graduate students in their dissertation efforts on the topic of the relationship of government to economic development. He was particularly interested in the promotion of railroads and canals during the mid-nineteeth century. His *Government Promotion of American Canals and Railroads* (1960) summarized much of his old work. In 1961 he and several of his students published another volume, *Canals in American Development*. Together the two books show the tremendous complexities in the relationship between government, private business, and economic development.

His work at the University of Pittsburgh generally focused on extending to other countries the investigation of these complex private, private/public, and public approaches to economic development. His seminar in comparative economic history, focusing in great part upon his own work in South America and Southeast Asia, was producing a flow of scholarly studies at the point of his untimely death.

Thoroughly respected and admired by his colleagues and his many students, Professor Goodrich epitomized a happy combination of an imaginative theorist, a careful empiricist, and a literary stylist. His final publication, "Bolivia in Time of Revolution," is included in *Beyond the Revolution* (1972), which has been posthumously dedicated to him.

MARK PERLMAN
University of Pittsburgh

The sudden death on July 15, 1971, of SAMUEL J. HURWITZ came as a great shock to his many friends, colleagues, and former students.

He graduated from Brooklyn College in 1934

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in the midst of the depression. Earning his living as director of research for the American Association for Social Security, he attended law school and secured his LL.B. He then decided to do graduate work in history at Columbia University, where he received his Ph.D. in 1946. His dissertation, *State Intervention in Great Britain, A Study of Economic Control and Social Response, 1914–1919* (1949), is still cited as the outstanding book on the subject on both sides of the Atlantic and is to be reprinted.

During this period he had begun his teaching career in the history department of Brooklyn College, first in the school of general studies and then in 1942 in the day session. He was a dedicated teacher, intellectually challenging and demanding of his students. Students' papers were slashed and rigorously criticized, gaining the benefit of his professional editorial work; many a former student, now a distinguished member of our profession, owes his skill in writing to this traumatic experience. Hurwitz's editorial skill and advice were also much appreciated and publicly acknowledged in the works of many of his colleagues across the country from Harvard to Berkeley as well as at Brooklyn. He was sympathetic and understanding of his students' problems. His warmth and generosity to students extended also to helping them with their personal career difficulties and even in some cases with loans from his own limited funds.

Despite the demands on his time of a heavy teaching load and his commitment to his students, he continued his research on modern Britain and published several articles, such as "Development of the Social Welfare State in Pre-War Britain," in Robert L. Schuyler and Herman Ausubel, eds., *The Making of English History* (1952). His study of Churchill as a historian in *Some Modern Historians of Britain* (1951) in the *Festschrift* for R. L. Schuyler, edited by Herman Ausubel, J. Barthel Brebner, and E. M. Hunt, was the beginning of his research for a biography of Churchill, which, however, was not completed. His interest extended beyond Britain to continental Europe, particularly in the nineteenth and twentieth centuries, and led to the preparation of encyclopedia articles and contributions to books and periodicals.

A Fulbright fellowship took him to the University of Puerto Rico in 1960–61, where he became interested in the Caribbean area and in the antislavery movement. A year at the University of the West Indies, 1963–64, gave him time for further research, which culminated in articles in the *Journal of British Studies,* the *American Jewish Historical Quarterly,* and the *William and Mary Quarterly,* and in a book written jointly with his wife Edith, *Jamaica: a Historical Portrait* (1971).

The lure of warmer climes took him to the University of Hawaii in 1967–69, and after a brief return to Brooklyn College he retired and accepted a visiting professorship at San Diego State College.

All during his career he continued to review for the *American Historical Review,* the *Journal of Economic History,* the *Political Science Quarterly,* and other historical journals. All his writings attest to his critical judgment, his appreciation of nuance, the breadth of his interest, and his sense of style.

His official service to Brooklyn College as adviser to student clubs, member of college committees, president of the local chapters of Phi Beta Kappa and the American Association of University Professors, deputy chairman of the department of history for the day session, the summer session, and the graduate division, all demonstrate his commitment to his students and to the college. He will be missed by many.

MADELINE R. ROBINTON
Brooklyn College

ADRIENNE KOCH died of cancer in New York City on August 21, 1971, less than a month before her fifty-ninth birthday. Born and educated in New York City, she spanned the continent in the course of her academic career. Having entered the world of learning as a student and teacher of philosophy, she left it as a professor of history. Her experience was broadened by service in the Office of War Information and other agencies during the Second World War. She was happily married in 1947 to Lawrence R. Kegan, an economist who was deeply appreciative of her scholarly work, and she was the mother of twins—a son and a daughter.

She had academic appointments at her alma mater, New York University, at Tulane, at the University of California, Berkeley (where she was chairman of American studies), and, after

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her husband's work called him to Washington from the Pacific Coast, at the University of Maryland. Between times she was visiting professor at the University of Michigan, lectured in Japan and at Princeton, was visiting scholar for Phi Beta Kappa, and served on important committees and advisory boards. Because of her personal magnetism, as well as her facility in public discourse, she was in great demand as a speaker. Following the amputation of a leg several years ago she was partially immobilized, but she showed her indomitable spirit by attending historical conventions on crutches.

A ready and prolific writer, she published half a score of books and two or three times that many essays and articles, only a few of which are mentioned here. In her doctoral dissertation, which won the Woodbridge Prize at Columbia and was published as *The Philosophy of Thomas Jefferson* (1943), she sought to systematize the thought of one whose expression of it was generally *ad hoc,* but she viewed it in no vacuum. That she was interested not merely in thought but in thinkers and doers was shown by her publication, as joint editor, of selected writings of Jefferson (1944) and of John and John Quincy Adams (1946), both of which books were useful compilations with stimulating introductions. She never ceased to be concerned with ideas, but in *Jefferson and Madison* (1950), a notably suggestive book that tapped fresh sources, she dealt with ideas as tested in action. She had arrived at the judgment that the reconciliation of high principle with essential practice by these and other philosophical statesmen provides an essential clue to the success of the American experiment in self-government. Some of her later writings—notably *Philosophy for a Time of Crisis* (1959)—bore directly on the thought problems of the day, but she viewed these in the light of the thought and actions of the leaders she most esteemed in the half century she liked best, 1765–1815. Just who these men were can be ascertained from her anthology, *The Age of the American Enlightenment* (1965). It may be noted, also, that among her last publications were her Princeton lectures on Madison (1966) and a little book on Jefferson (1971). At the time of her death, however, she was engaged in a study of the Grimké sisters of South Carolina. We can only speculate how this humanistic admirer of eighteenth-century reasonableness and balance would have interpreted nineteenth-century abolitionism.

In Adrienne Koch there was a vibrancy and verve that are rare among scholars. She could be severe in criticism but did not hesitate to be enthusiastic in praise. No one who really knew her is at all likely to forget the warmth, the vigor, and the richness of her personality.

DUMAS MALONE
Alderman Library,
University of Virginia

CHARLES LOCH MOWAT died on June 23, 1970. The unseemly delay in this acknowledgment of our profession's loss can be explained only by the dispersal a week earlier of the faculty of the last American university he graced, Chicago, and undue delay in finding a contemporary to express our regrets. Time has not softened the sense of personal loss of those who knew him.

He was born on October 4, 1911, at Oxford, first son of a famous historian, Robert Balmain Mowat, Fellow of Corpus Christi. It is not unique for a historian's son to follow his father's calling, but it is more rare than in other professions, such as medicine, law, or politics. In this case it was a fortunate exception.

English sources report that Charles was "educated at Marlborough and St. John's College, Oxford" (B.A. 1934, M.A. 1937). In 1934 he came to the United States and until 1936 served as a teaching assistant in history at the University of Minnesota while working on his graduate program. One may hope that some part of his "education" was received in one of the best of our huge state universities. He received the Ph.D. in 1939 upon completion of a dissertation entitled "East Florida under English Rule, 1763–83."

Thereafter his career was typical of the young American college teacher. Before receiving the doctorate he had joined the faculty of the University of California, Los Angeles, where he taught from 1936 to 1950, advancing rank by rank—associate, instructor, assistant professor. He taught summers at Montana State University, Santa Barbara College, and at Berkeley. In 1947–48 he was a Guggenheim Memorial fellow. Normally one would have expected Mowat to have gone on as associate professor with tenure, but this was the age of Joe McCarthy and the

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McCarran Act, and in 1950 the regents of the University of California demanded that all faculty members sign a loyalty oath. Most refused to sign the original oath, but when a milder version was substituted with a "sign or resign" threat, Mowat was one of the relatively few nonjurors.

Fortunately for our department, Mowat was teaching at the University of Chicago that summer (1950). He was offered, and accepted, a position as associate professor with tenure and was promoted to full professor in 1956. He taught courses in modern British history, predominantly or perhaps exclusively to graduate students. Mowat had taken out citizenship papers; he was interested in the social and political life of his adopted country, and since his own graduate work and all his teaching had been in the States, most of us thought he would round out his career here, but in the end early home ties won out. He had been a Fulbright lecturer in history at University College of Swansea in 1955–56, and in 1959 he moved to a professorship at the University of North Wales, Bangor, where he taught until he died. He did return for a summer's teaching at Chicago in 1965.

Mowat's publications centered chiefly around two disparate topics: English colonialism in eighteenth-century Florida and Britain in the late nineteenth and twentieth centuries. The former interest, obviously the offspring of his dissertation, is represented by a like-named volume published in 1943 and a flock of articles. Later books included *The Charity Organization Society, 1869–1913* (1961), *The Golden Valley Railway* (1964), and *The General Strike, 1926* (1967). But certainly his most important work was *Britain between the Wars, 1918–1940* (1955), which became, one may suppose, the best known —and the best—general treatment of that period of vast changes. Mowat's works showed a thorough grasp of the sources and a scholarly balance in his judgments. He wrote with serene clarity, no matter how obscure his problem and his sources. I recall a paper on Wales he read at a faculty seminar, a piece I have not seen in print. His comments on the land, its people, and its history were almost poetic and left one with the feeling that he now had a grasp of the very essence of Wales. Mowat also wrote sundry papers on current problems of education and politics, both American and British.

His scholarly and literary talents were rewarded by various appointments. At Chicago he was editor of the *Journal of Modern History* (1956–59). He was chosen to edit the second edition of volume 12 of the *New Cambridge Modern History* (1968). He served on the council of the Royal Historical Society from 1969. In March 1970 he was elected president of the Historical Association but died before taking office.

Mowat had all the requirements for a successful academic administrator, and one wonders at his good fortune in being allowed to remain with the professor's chief responsibilities—research and teaching. To him teaching was not a bore but another means of communicating substantive knowledge while showing the graduate student the nature of the historian's craft. Students found him easily approachable; we on the staff found him an ideal colleague in conducting the teaching program. Certainly in the confusion and violence that were so prevalent in recent years his courage, his integrity, and his good sense would have been at a premium on many a troubled campus.

Charles left a widow, a daughter, and a son. We who were priviledged to know him intimately share in a lesser degree their grief.

JAMES LEA CATE
University of Chicago

JEANNE MELCHIOR PROSSER, assistant professor of history at Dartmouth College, died on August 21, 1971, at the age of forty-three. Dr. Prosser was a 1950 graduate of Radcliffe College, where she won the junior prize in history and literature. She was also a Fulbright scholar and received a Doctorat des lettres de l'Université from the University of Bordeaux. Dr. Prosser taught French at Tufts University from 1962 to 1965 and was a Fellow of the Radcliffe Institute for Independent Studies from 1965 to 1966, when she joined the Dartmouth faculty. At Dartmouth she engaged in a major computer-assisted study of the social and economic conditions of Bordeaux on the eve of the French Revolution.

THEODORE A. THELANDER, JR., professor of history at Indiana University-Purdue University, Indianapolis, died on August 25, 1971. He received his Ph.D. at the University of Wisconsin in 1940. He taught at the John Burroughs

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School in St. Louis and the University of Texas before moving to Purdue University's Indianapolis campus in 1947. There he was twice recognized by student and faculty awards for his distinguished teaching. Mr. Thelander, a diabetic who suffered increasing physical complications, used canes, crutches, and a wheel chair as he conducted his classes and carried a full load of departmental and institutional responsibilities. He completed a summer session one week before his death. He taught a wide range of courses in history and political science and, although his training had been in classical and medieval history and these remained his fields, his research and publication were on technical aspects of aeronautical and automotive development and on the definition and emergence of the related engineering professions.

PATTY WILLIS WASHINGTON, for many years assistant secretary-treasurer of the American Historical Association, died February 22, 1971, in Alexandria, Virginia. She was born October 1, 1880, at Waveland, the family seat in Fauquier County, Virginia. Her grandfather, John Augustine Washington, was the last member of the family to retain ownership of Mount Vernon.

Miss Washington's long term of service, extending from 1908 to her retirement in 1956, constitutes a record in the history of the association. During that period of almost half a century she had come to know in a detailed way more about the association and its activities than anyone else. Even after her retirement, when some needed item of information could not be located, a telephone call to Miss Washington would invariably yield what was wanted. No one had briefed her on these things, for she was there herself, and her knowledge was firsthand.

During the first half of her tenure Dr. J. Franklin Jameson was a dominant influence in the affairs of the association. Miss Washington was Jameson's chief of staff and the better part of the staff as well. The two made a good team, a team of real dignity. Jameson's dignity was of the austere type, Miss Patty's that of the gracious lady, with an unfailing sense of humor that could even dent the Jamesonian armor.

During most of her years the association was housed in borrowed and cramped quarters. It was only as she approached the time of her retirement that steps were taken to acquire a building and to house the association's headquarters under its own roof. In the early decades there was but little staff and Miss Washington, having to be Jack-of-all-trades, became master of them all. She saw the membership grow from a few hundred to several thousands. She looked on with amazed wonder as the annual meeting changed from a gathering of a modestly small group enjoying pleasant social occasions and professional discussions to a vast convention drawing thousands.

In a very real way, Patty Washington dedicated her life to the American Historical Association. A woman of sound intelligence, of great loyalty and high integrity, and of warm human understanding, she formed a continuum between the old and the new in a period of great growth in the association's membership, influence, and prestige. She was exacting in the standards that she imposed on herself in doing the association's work, and the results of her labor will long remain.

Recalling the orderliness of her mind, her strong historical sense, and the name that she bore, it would almost seem the divine fulfillment of her own planning that she passed away on February 22.

ELMER LOUIS KAYSER
American Historical Association

GEORGE WASKOVICH, professor emeritus of history at Hunter College, died September 30, 1971, at his home in Brooklyn, New York. He was seventy-five years old. Born in Greenwich, Connecticut, Dr. Waskovich received his bachelor's and master's degrees from Tufts University and his Ph.D. from Harvard. He taught at St. Theresa College in Winona, Minnesota, and at St. John's University before joining the Hunter faculty.

CARL WITTKE (November 13, 1892–March 24, 1971) was one of the first generation of historians of American immigration, in company with Marcus L. Hansen, Arthur M. Schlesinger, George M. Stephenson, and Theodore C. Blegen. These pioneers rescued ethnic history from the filiopietists and made it a scholarly field. Although his first book was in the field of English history, and even though his *History of Canada* (1928) gave him distinction in Canadian history, Wittke's primary scholarly contributions were on the history of immigration to

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the United States. In his major work in this field, *We Who Built America* (1939), after paying tribute to his immigrant parents, he stated his thesis: "In spite of all the discouraging obstacles that beset the scholar who tries to thread his way through the complex history of immigrant contributions to America, the fact remains that the grand central motif of United States history has been the impact of successive immigrant tides upon a New World environment, or the interaction of so-called 'racial,' or immigrant, characteristics with the forces of American geography" (p. xii). This Turnerian interpretation, despite de-emphasis on contributions and modifications by demographers and behaviorists, remains a valid one.

A virtually complete list of Carl Wittke's publications, together with an eloquent tribute by his colleague, the late Harvey Wish, is to be found in the *Festschrift* published in his honor on the occasion of his retirement from Case Western Reserve University (*In the Trek of the Immigrants* [1965]). While *We Who Built America* is a comprehensive survey of immigrant groups, excepting the English on whom little had been published at that time, his other books and articles on immigration were more specialized. His studies of the German intellectuals who fled to the United States after the Revolution of 1848 were definitive: *Refugees of Revolution* (1952), *Karl Heinzen* (1945), and *Wilhelm Weitling* (1959). *The Irish in America* (1956) complemented W. F. Adams's volume on Irish emigration. *The German Language Press in America* (1957) filled a gap in our knowledge of that leading feature of German-American life. These and other studies gained for Carl Wittke enduring stature in immigration history.

The versatility of Carl Wittke impressed all who knew him. He could write a delightful *Tambo and Bones: A History of the American Minstrel Stage* (1930), fortunately reprinted. He edited and contributed to a monumental six-volume history of the state of Ohio and was editor of a publisher's history series. He served on the editorial boards of the *Mississippi Valley Historical Review* (now the *Journal of American History*), the *Canadian Historical Review*, and the *Ohio Archeological and Historical Review*. He was thrice dean: of the graduate school of Ohio State University, of Oberlin College, and of the graduate school of Case Western Reserve University. He was deservedly the recipient of several honorary degrees and was made an honorary member of the Deutsche Akademie and a Fellow of the Royal Historical Society. From his school days in Columbus and at Ohio State University through the graduate years at Harvard University until his retirement, Carl Wittke maintained an enviable level of distinguished scholarship, teaching, and service. In the memories of his family, friends, associates, and students, Carl Wittke will have personal immortality. For the historical profession his life's work should be an inspiration, and it is a permanent treasure.

CARLTON C. QUALEY
Cleveland State University

On March 11, 1971, there died, in the Oxford that he loved, SIR ERNEST LLEWELLYN WOODWARD, professor emeritus of the Institute for Advanced Study at Princeton and formerly professor, successively, of international relations and of modern history at Oxford. With his passing the Anglo-American scholarly world lost not only a scholar of great capacity in the field of modern history generally but also one of its finest and most distinguished diplomatic historians.

Llewellyn Woodward was born on May 14, 1890, into a middle-class London family, his father being a civil servant in the Naval Ordinance Department. From the old Merchant Taylors' School in London he went on to Corpus Christi College at Oxford. He completed his studies there in 1913 and became, shortly thereafter, a Senior Scholar at St. John's. Such was his modesty that the reader of his memoirs—*Short Journey*, published in 1942—would, unless well acquainted with the conditions of that day, never suspect that for a youth without money or patronage this progression was neither common nor easy.

A year of private study in Paris (1913–14) came to an end with the outbreak of the First World War. Woodward spent most of the war years in uniform, first in France (a country for which he conceived an abiding admiration and affection), then in Salonika. Returning to Oxford in 1919 he became a regular Fellow of All Souls—a status he was to hold until 1944. This was soon supplemented by a lecturership at New College in the field of modern history. For

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some two decades his life revolved mainly between these two neighboring institutions: New College, where he taught, and All Souls, where he held for some years the post of domestic bursar. At various times in his life he was also a member of several other Oxford common rooms, notably that of Worcester College, where he had an enduring set of friendships and connections.

In 1944 Woodward became Montague Burton Professor of International Relations at Oxford, a chair that he held until 1947, when he exchanged it for the Regius chair in modern history. In 1951 he accepted a call to the Institute for Advanced Study, where he remained, as a regular member of its faculty, until his retirement in 1960. Meanwhile he functioned, in the years 1944 to 1955, as one of the two founding editors (with Rohan Butler) of the official series of British diplomatic papers: *Documents on British Foreign Policy, 1919–1939*. He was knighted in 1952. In 1962, following his retirement from the Institute for Advanced Study, he became, and remained to his death, a Distinguished Fellow of All Souls.

Woodward's first published work appeared in 1915: a small study in Roman history—*Christianity and Nationalism in the Later Roman Empire*, a work of such originality and quality that it has not lost its interest for scholars to the present day. In the 1920s his energies were absorbed with administrative and teaching duties, but the reading and lecturing done in this period laid the groundwork for an impressive burst of scholarly publication in the ensuing decade. *Three Studies in European Conservatism*, dealing with Metternich, Guizot, and nineteenth-century Catholicism, appeared in 1930 and instantly established his reputation as a historian of outstanding competence. *War and Peace in Europe, 1815–1870* (1931), a volume based on certain of his Oxford lectures, included the excellent historiographic treatise entitled "Historical Material and Historical Certainty in the XIX Century." *French Revolutions* (1934), based on lectures delivered in Dublin, analyzed with charm and penetration the various upheavals in French political life from the late eighteenth century to the late nineteenth.

In the early 1930s, with the specter of Hitler's Germany now dominating the diplomatic horizon, Woodward turned to a work that he vainly hoped would serve as a warning to the Western statesmen of that period: a study of the international implications of the German naval construction program of the years just before World War I: *Great Britain and the German Navy* (1935). This book represented his first excursion into diplomatic history per se and into the area of British foreign policy in particular. It was followed in 1938 by a major work in an earlier period of English history: *The Age of Reform, 1815–1870*, published as a volume in the *Oxford History of England*. Despite the preoccupation with recent diplomatic history that was to dominate the remainder of his career, Woodward never lost his interest in earlier periods of English history; when in 1947 he consented to write a brief *History of England* for the Home Study Books series he did so, as he later said, almost entirely from memory and with keenest pleasure.

The years from 1944 to 1955 were devoted mainly to work on the *Documents on British Foreign Policy*. Some idea of the magnitude of this effort may be gained from the fact that of the forty volumes of this fine collection that have appeared to date, twenty-one were prepared under his coeditorship. In addition to this he produced during the years 1942 to 1950 the first volume, carrying the narrative to the German attack on Russia in 1941, of a semi-official history of *British Foreign Policy during the Second World War*—a study that was published for the first time in full only in 1970, shortly before his death.

Woodward's historical writing was always restrained, precise, and finely chiseled. When he allowed himself more liberty, as he did in some of his historical writing but primarily in his memoirs, articles, and addresses on other subjects, what he wrote was marked by unusual grace and beauty of expression and revealed a literary talent of a high order.

Solitary in habit, reserved and undemonstrative in personality, Woodward was nevertheless an engaging companion and a loyal, helpful colleague. Immensely saddened by the death in 1961 of his wife, his companion of fifty years in a childless but intensely devoted marriage, he spent the last years of his life in retirement in Oxford. He had always had a deep interest in religion; and while it is primarily his work

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in diplomatic history that will commend his name to the scholars of the future, some of those who knew him best will remember him perhaps most vividly from the remarkable address that he delivered, at the age of seventy-nine, in the chapel of All Souls. In this moving statement he exposed the doubts and questionings he had long experienced, as a historian, with relation to the historicity of the Christian gospel and the future of the Church, but associated himself with what he called the *non sequitur* of Job's final confession of faith. "Less magnificently," he said, "I have searched in the four quarters of heaven, and have not found God, yet I too am sure he knows the way I take."

GEORGE KENNAN
Institute for Advanced Study

Other members of the Association who have died recently include: Hardin Craig, Jr., of Houston, Texas; Louise P. Holden of Chelmsford, Massachusetts; the Reverend J. V. Jacobsen, S. J., of Chicago, Illinois; M. L. Koenig of Cedar Rapids, Iowa; M. Levey of Albany, New York; L. P. Wallace of Meredith College in Raleigh, North Carolina; and Karl Wernert of Utica, New York.

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Association Notes

Dr. Dorothy Rabin Ross has joined the staff of the American Historical Association as special assistant to the AHA's Committee on Women Historians.

Miss Sally L. Banks has resigned as assistant editor of the *American Historical Review* to join the staff of the National Education Association. Miss Robin E. Byrnes has been appointed as editorial assistant.

This issue contains the last lists of recently published articles compiled by Professor T. Robert S. Broughton, in ancient history, and Dr. Cecil Hobbs, in South Asian history. Both scholars are retiring from their academic posts at the University of North Carolina, Chapel Hill, and at the Library of Congress, respectively. Particular note must be taken of the long service of Professor Broughton. He has been compiling lists for the *AHR* since the summer of 1930 and has done so with a fidelity to the highest standards of bibliographical work that has put all his fellow scholars in his debt. Professor Broughton's responsibilities will be taken over by Professor George W. Houston, a former student of Professor Broughton's and also a member of the classics department at Chapel Hill. Dr. Hobbs's responsibilities are being divided. The South Asian field will be covered by Professor Stephen Hay of the University of California, Santa Barbara, while articles published in the history of Southeast Asia will be listed by Professor D. R. SarDesai of the University of California, Los Angeles.

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Festschriften and Miscellanies

These volumes, recently received in the *AHR* office, do not lend themselves readily to unified reviews; the contents are therefore listed. Other *Festschriften* and similar volumes that are amenable to reviewing will be found in the review section.

BUCHANAN, R. H., *et al.*, editors. *Man and his Habitat: Essays presented to Emyr Estyn Evans.* New York: Barnes & Noble. 1971. Pp. xi, 279. $11.50.

H. J. FLEURE, Emyr Estyn Evans: A personal note. BRUCE PROUDFOOT, Man's occupance of the soil. CARL O. SAUER, Plants, animals and man. AXEL STEENSBERG, Tools and man. JOHN MOGEY, Society, man and environment. HARALD UHLIG, Fields and field systems. DESMOND MCCOURT, The dynamic quality of Irish rural settlement. P. FLATRÈS, Hamlet and village. E. G. BOWEN, The dispersed habitat of Wales. J. K. WRIGHT, Mountain glory and mountain gloom in New England. ANDREW CLARK, The Canadian habitat. EMRYS JONES, The future habitat. M. L. HENRY, compiler, A Bibliography of the writings of E. Estyn Evans.

KING, LESTER S., editor. *Mainstreams of Medicine: Essays on the Social and Intellectual Context of Medical Practice.* Austin: University of Texas Press for the University of Texas Medical School at San Antonio. 1971. Pp. x, 186. $6.50.

DAVID A. KRONICK, Introduction: Medical Education in Mainstream. CHAUNCEY D. LEAKE, Great Medical Practitioners: A Historical Survey. GEORGE ROSEN, Medicine as a Function of Society. RENÉ DUBOS, The Diseases of Civilization: Achievements and Illusions. JOHN H. KNOWLES, Emergence of the Hospital as a Social Institution. DAVID D. RUTSTEIN, The Coming Revolution in Medicine: A New Plan for Ambulatory Medical Care. THOMAS HALE HAM, A Clinical Investigator Looks at Modern Education: The Discovery of the Medical Student as a Responsible Colleague. HARRY F. DOWLING, The Impact of New Discoveries on Medical Practice: Advances in the Diagnosis and Treatment of the Infectious Diseases. DOUGLAS D. BOND, Changing Concepts of Deviance. LESTER S. KING, The Development of Scientific Medicine. HENRY K. BEECHER, Ethical Problems of Medical Research.

Liber Memorialis Georges de Lagarde. (Studies presented to the International Commission for the History of Representative and Parliamentary Institutions, Volume 38, London, 1968.) Louvain: Éditions Nauwelaerts. 1970. Pp. xvi, 404, 46. 950 fr. B.

EMILE SZLECHTER, Les Assemblées en Mésopotamie Ancienne. WALTER MOHR, Die Auswirkung des Gewaltenstreites im Hochmittelalter auf die Entwicklung von Repräsentations-Ideen. MARIO GRIGNASCHI, L'interprétation de la "Politique" d'Aristote dans le "Dialogue" de Guillaume d'Ockham. EDMUND B. FRYDE, English Parliament and the Peasants' Revolt of 1381. LEO E. M. A. VAN HOMMERICH, La Politique centralisatrice de Charles le Téméraire à l'égard des Assemblées d'états du Duché de Limbourg et des autres Pays d'Outre-Meuse, 1469–1477. VALENTIN AL. GEORGESCO, Types et Formes d'Assemblées d'états en Droit féodal roumain. MICHAEL G. KAMMEN, The Origins of Representative Government in British North America: An Interpretive Inquiry. GEORGE L. HASKINS, Parliamentary Aspects of Representative Government in Early Massachusetts. PÄR E. BACK, Diets in Ingria in the Seventeenth Century. SVEN ULRIC PALME, The Bureaucratic Type of Parliament: The Swedish Estates during the Age of Liberty, 1719–1772. RUDOLFINE FREIIN VON OER, Estates and Diets in Ecclesiastical Principalities of the Holy Roman Empire (18th Century). DIETRICH GERHARD, Assemblies of Estates and the Corporate Order. WOLFGANG HOFMANN, Prussian Town Councils in the 19th Century as Representative Institutions. MARC SZEFTEL, The Reform of the Electoral Law to the State Duma on June 3, 1907: A New Basis for the Formation of the Russian Parliament. GEOFFREY J. HAND, The Parliament contemplated by the Irish

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Home Rule Act of 1914. MARTIN VIETOR, Der Slovakische Nationalrat.

STEVENSON, LLOYD G., and MULTHAUF, ROBERT P., editors. *Medicine, Science, and Culture: Historical Essays in Honor of Owsei Temkin.* Baltimore: Johns Hopkins Press. 1968. Pp. xvii, 312. $10.00.

ERWIN H. ACKERKNECHT, The Vagaries of the Notion of Epidemic Hepatitis or Infectious Jaundice. GEORGE ROSEN, Some Notes on Greek and Roman Attitudes toward the Mentally Ill. FRIDOLF KUDLIEN, The Third Century A.D.—A Blank Spot in the History of Medicine? MARSHALL CLAGETT, Johannes de Muris and the Problem of Proportional Means. WALTER PAGEL, Paracelsus: Traditionalism and Medieval Sources. C. D. O'MALLEY, Gabrielle Falloppia's Account of the Orbital Muscles. SAUL JARCHO, Morgagni, Vicarius, and the Difficulty of Clinical Diagnosis. F. N. L. POYNTER, Hunter, Spallanzani, and the History of Artificial Insemination. GENEVIEVE MILLER, Leeuwenhoek's Observations on the Blood and Capillary Vessels. EDWIN CLARKE, The Doctrine of the Hollow Nerve in the Seventeenth and Eighteenth Centuries. LESTER S. KING, Medical Philosophy, 1836–1844. WILLIAM L. STRAUS, JR., Huxley's *Evidence as to Man's Place in Nature*—A Century Later. JOHN B. BLAKE, Anatomy and the Congress. VICTOR A. MCKUSICK, The Ethnicity of Disease in the United States: The Historical and Sociologic Basis of the Distribution of Rare Recessive Disorders. BENTLEY GLASS, Evolution and Heredity in the Nineteenth Century. GEORGE W. CORNER, Hero with a Damaged Heart: The Clinical History of Elisha Kent Kane. WHITFIELD J. BELL, JR., James Hutchinson (1752–1793): A Physician in Politics. RICHARD H. SHRYOCK, A Doctor in Public Life: Richard D. Arnold of Savannah (1808–1876). JANET B. KOUDELKA, compiler, Published Writings of Owsei Temkin.

STUEWER, ROGER H., editor. *Historical and Philosophical Perspectives of Science.* (Minnesota Studies in the Philosophy of Science, Volume 5.) Minneapolis: University of Minnesota Press. 1970. Pp. xix, 384. $11.50.

HERBERT FEIGL, Beyond Peaceful Coexistence. ERNAN MCMULLIN, The History and Philosophy of Science: A Taxonomy. WESLEY C. SALMON, Bayes's Theorem and the History of Science. PETER ACHINSTEIN, Inference to Scientific Laws. ARNOLD THACKRAY, Science: Has Its Present Past a Future? MARY HESSE, Hermeticism and Historiography: An Apology for the Internal History of Science. EDWARD ROSEN, Was Copernicus a Hermetist? PAUL K. FEYERABEND, Philosophy of Science: A Subject with a Great Past. ERWIN N. HIEBERT, Mach's Philosophical Use of the History of Science. GERD BUCHDAHL, History of Science and Criteria of Choice. ROGER H. STUEWER, Non-Einsteinian Interpretations of the Photoelectric Effect. HOWARD STEIN, On the Notion of Field in Newton, Maxwell, and Beyond. KENNETH F. SCHAFFNER, Outlines of a Logic of Comparative Theory Evaluation with Special Attention to Pre- and Post-Relativistic Electrodynamics.

WANDYCZ, DAMIAN S., editor. *Studies in Polish Civilization: Selected Papers Presented at the First Congress of the Polish Institute of Arts & Sciences in America, November 25, 26, 27, 1966 in New York.* [New York:] Institute on East Central Europe, Columbia University, and the Polish Institute of Arts & Sciences in America. 1969. Pp. xi, 13–549. $7.50.

History: OSCAR HALECKI, Poland's Place in Europe 966–1966. BOLESLAW B. SZCZEŚNIAK, The Imperial Coronation of Gniezno in A.D. 1000. ISAAC LEWIN, The Historical Background of the Statute of Kalisz. DANIEL BUCZEK, Archbishop Jakub Świnka 1283–1314. OTAKAR ODLOZILIK, Andrzej Rej and His French Contacts. IRENE SOKOL, The American Revolution as Seen in Eighteenth Century Poland. PETER BROCK, A Pacifist in Wartime: Wojciech Jastrzębowski. STANISŁAW BÓBR-TYLINGO, Lord Clarendon's Mission to Germany in 1863. JOSEPH ROTHSCHILD, A Chapter in Polish Politics of the 1920s. THADDEUS V. GROMADA, Poland and Slovakia During the September–October Crisis of 1938. JAN LIBRACH, The Łukasiewicz Papers. WACŁAW W. SOROKA, The Anticipation of the Second World War by the Polish Peasant Leaders. JOSEPH L. LICHTEN, Some Aspects of Polish-Jewish Relations During the Nazi Occupation. WALTER M. DRZEWIECKI, The New "Historia Polski" of the Polish Academy of Science.

Literature: EDWARD J. CZERWIŃSKI, The Polish Theater of the Absurd. ABRAHAM G. DUKER, Some Cabbalistic and Frankist in Adam Mickiewicz's "Dziady." MIECZYSŁAW GIERGIELEWICZ, Americana of Bolesław Prus. JERZY R. KRZYŻANOWSKI, The Promised Land: A Modern Novel. GEORG W. STROBEL, The Journey of Adam Mickiewicz Through Germany in 1832. TYMON TERLECKI, A Critical Appraisal of Mickiewicz's Lecture About the Theater. WIKTOR WIENTRAUB, Three Myths of America in Polish Romantic Literature.

Economics and Technology: WILLIAM A. DYMSZA, United States Policy on East-West Trade. STANLEY J. GLOD, Poland's Role in East-West Trade. STANISŁAW SKRZYPEK, Soviet Economic Aid to the East European Nations 1955–1962: An Evaluation. W. J. KLIMKIEWICZ, The Polish Petroleum Industry from its Inception to 1909.

Law, Political Science, Geopolitics: STEFAN K. JANKOWSKI, Early Polish Habeas Corpus Act. STEFAN KORBOŃSKI, Underground Administration of Justice During the German Occupation of Poland 1939–

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1945. ALEKSANDER W. RUDZIŃSKI, Is There a Specific Socialist Marriage Law? HELEN SILVING, Six Million Martyrs. WIKTOR SUKIENNICKI, Political Consequences of a Semantic Mistake. MILOS M. SEBOR, Geographic Aspects of the Cieszyn Case.

Sociology and Philosophy: THEODORE ABEL, Social Science in Poland. HENRYK HIŻ, Kotarbiński on Truth.

Medicine: JOSEPH A. MEZYK, On Certain Medical Problems Encountered at the German Concentration Camp Auschwitz 1940–1945. ALEXANDER RYTEL, Achievements of Polish Physicians in the Nineteenth Century. S. H. WAJDA, The Cracow Anatomical School.

Fine Arts: PRIVA B. GROSS, Some Aspects of Medieval Architecture in Poland. JADWIGA IRENA DANIEC, The Bronze Door of the Gniezno Cathedral.

Abstracts of Papers Delivered at the Congress but not included in this Volume.

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Other Books Received

Books listed were received by the *AHR* between August 1 and October 1, 1971. Books that will be reviewed are not listed, but listing does not preclude subsequent review.

GENERAL

ACKERKNECHT, ERWIN H. *Medicine and Ethnology: Selected Essays*. Ed. by H. H. WALSER and H. M. KOELBING. Baltimore: Johns Hopkins Press. 1971. Pp. 195.

ALLSOPP, BRUCE. *The Study of Architectural History*. Books That Matter. [New York:] Praeger. 1970. Pp. 128. Cloth $7.50, paper $3.95.

ALTHUSSER, LOUIS, and BALIBAR, ÉTIENNE. *Reading Capital*. Tr. from the French by BEN BREWSTER. New York: Pantheon Books. 1970. Pp. 340. $10.00.

BEIDELMAN, T. O. (ed.). *The Translation of Culture: Essays to E. E. Evans-Pritchard*. [London:] Tavistock Publications; distrib. by Barnes and Noble, New York. 1971. Pp. ix, 440. $18.50.

BIRNBAUM, NORMAN. *Toward a Critical Sociology*. New York: Oxford University Press. 1971. Pp. xiv, 451. $10.95.

BONDURANT, JOAN V., in association with MARGARET W. FISHER (eds.). *Conflict: Violence and Nonviolence*. Chicago: Aldine Atherton. 1971. Pp. xiv, 206. Cloth $7.95, paper $2.95.

BOOKCHIN, MURRAY. *Post-Scarcity Anarchism*. Berkeley: Ramparts Press. 1971. Pp. 288. Cloth $6.95, paper $2.95.

BOZEMAN, ADDA B. *The Future of Law in a Multicultural World*. Princeton: Princeton University Press. 1971. Pp. xvii, 229. Cloth $6.50, paper $2.45.

COLTHAM, JEANETTE B. *The Development of Thinking and the Learning of History*. Teaching of History Ser., No. 34. London: Historical Association. 1971. Pp. 44. 30p.

CUNLIFFE, MARCUS (ed.). *The London Times History of Our Times*. New York: W. W. Norton. 1971. Pp. 416. $20.00.

DIVINE, ROBERT A. (ed. with commentary). *The Cuban Missile Crisis*. Chicago: Quadrangle Books. 1971. Pp. 248. Cloth $8.95, paper $2.65.

L'ETANG, HUGH. *The Pathology of Leadership*. New York: Hawthorn Books. 1970. Pp. 218. $6.95.

FLITNER, WILHELM. *Die Geschichte der abendländischen Lebensformen*. Piper Paperback. Reprint; Munich: R. Piper & Co. Verlag. 1967. Pp. 368. DM 16.80.

GALBRAITH, JOHN KENNETH. *The New Industrial State*. 2d rev. ed.; Boston: Houghton Mifflin. 1971. Pp. xxii, 423. $8.95.

GOLLIN, JAMES. *Worldly Goods: The Wealth and Power of the American Catholic Church, the Vatican, and the Men Who Control the Money*. New York: Random House. 1971. Pp. xxxiv, 531. $8.95.

HAZARD, LELAND. *Law and the Changing Environment: The History and Processes of Law*. San Francisco: Holden-Day. 1971. Pp. xxv, 454. $11.95.

JAY, ANTONY. *Corporation Man: Who He Is, What He Does, Why His Ancient Tribal Impulses Dominate the Life of the Modern Corporation*. New York: Random House. 1971. Pp. xii, 304. $7.95.

KAISER, KARL, and MORGAN, ROGER (eds.). *Britain and West Germany: Changing Societies and the Future of Foreign Policy*. New York: Oxford University Press for the Royal Institute of International Affairs. 1971. Pp. x, 294. $13.00.

KIRCHNER, WALTHER. *Einige Bemerkungen über die Quellenlage für quantitative Studien der frühen Neuzeit*. Kölner Vorträge zur Sozial- und Wirtschaftsgeschichte, No. 15. Cologne: Forschungsinstitut für Sozial- und Wirtschaftsgeschichte an der Universität zu Köln. 1971. Pp. 27.

LAQUEUR, WALTER (ed.). *A Dictionary of Politics*. New York: Free Press. 1971. Pp. 593. $14.00.

MILLER, NORMAN, and AYA, RODERICK (eds.). *National Liberation: Revolution in the Third World*. With an introd. by ERIC R. WOLF. New York: Free Press. 1971. Pp. xxvi, 307.

NICHOLSON, MICHAEL. *Conflict Analysis*. New York: Barnes and Noble. 1971. Pp. viii, 168. $8.00.

NICHOLSON, T. R. *Passenger Cars 1905–1912*. Cars of the World in Color. [New York:] Macmillan. 1971. Pp. 162. $3.95.

ROSEN, EDWARD (tr. with introd. and notes). *Three Copernican Treatises: The* Commentariolus *of Copernicus; The* Letter against Werner; *The* Narratio prima *of Rheticus*. 3d ed., rev., with a biography of Copernicus and Copernicus bibliographies, 1939–1958 and 1959–1970. Records of Civili-

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zation: Sources and Studies, No. 30. New York: Octagon Books. 1971. Pp. 425. $20.00. See rev. of 1st ed. (1939), *AHR*, 46 (1940–41): 624.

RUDÉ, GEORGE. *Paris and London in the Eighteenth Century: Studies in Popular Protest.* New York: Viking Press. 1971. Pp. 350. $8.95.

SCHON, DONALD A. *Beyond the Stable State.* New York: Random House. 1971. Pp. 254. $7.95.

SEROFF, VICTOR. *The Real Isadora.* New York: Dial Press. 1971. Pp. 441. $10.00.

STEINER, GEORGE. *In Bluebeard's Castle: Some Notes towards the Redefinition of Culture.* New Haven: Yale University Press. 1971. Pp. 141. $5.95.

TOYNBEE, ARNOLD. *Surviving the Future.* New York: Oxford University Press. 1971. Pp. xii, 164. $5.95.

TURNEY-HIGH, HARRY HOLBERT. *Primitive War: Its Practice and Concepts.* Foreword by DAVID C. RAPOPORT. Afterword by HARRY HOLBERT TURNEY-HIGH. 2d ed.; Columbia: University of South Carolina Press. 1971. Pp. xv, 288. $9.95.

ANCIENT

ALDRED, CYRIL. *Egypt: The Amarna Period and the End of the Eighteenth Dynasty.* Rev. ed. of *The Cambridge Ancient History,* Vol. 2, ch. 19. New York: Cambridge University Press. 1971. Pp. 59. $1.45.

Bibliografia di storia antica e diritto romano. Rome: "L'Erma" di Bretschneider. 1971. Pp. 257. L. 3,000.

FÖDISCH, HERMANN. *Zum Problem präurbaner Siedlungen in Ostmitteleuropa.* Wissenschaftliche Materialien und Beiträge zur Geschichte und Landeskunde der böhmischen Länder, No. 7. Munich: Verlag Robert Lerche. 1967. Pp. 116. DM 10.

HALLO, WILLIAM W., and SIMPSON, WILLIAM KELLY. *The Ancient Near East: A History.* New York: Harcourt Brace Jovanovich. 1971. Pp. x, 319. $4.95.

The Linear Scripts and the Tablets as Historical Documents. (A) *Literacy in Minoan and Mycenaean Lands,* by STERLING DOW, and (B) *The Linear B Tablets as Historical Documents,* by JOHN CHADWICK. Rev. ed. of *The Cambridge Ancient History,* Vol. 2, ch. 13. New York: Cambridge University Press. 1971. Pp. 51. $1.75.

MICHEL, ALAIN. *Histoire des doctrines politiques à Rome.* "Que sais-je?" No. 1442. Paris: Presses Universitaires de France. 1971. Pp. 127.

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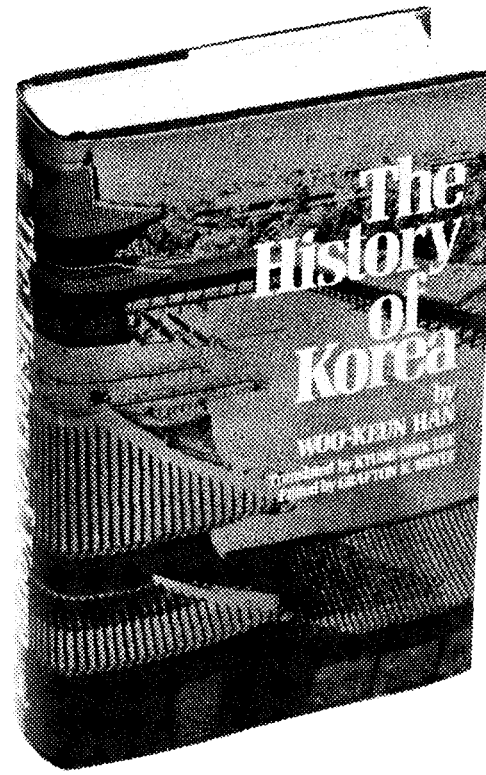
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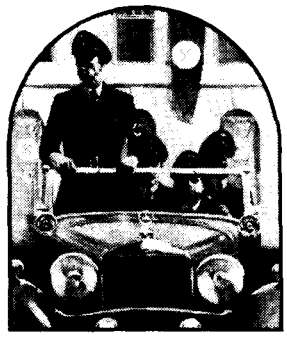
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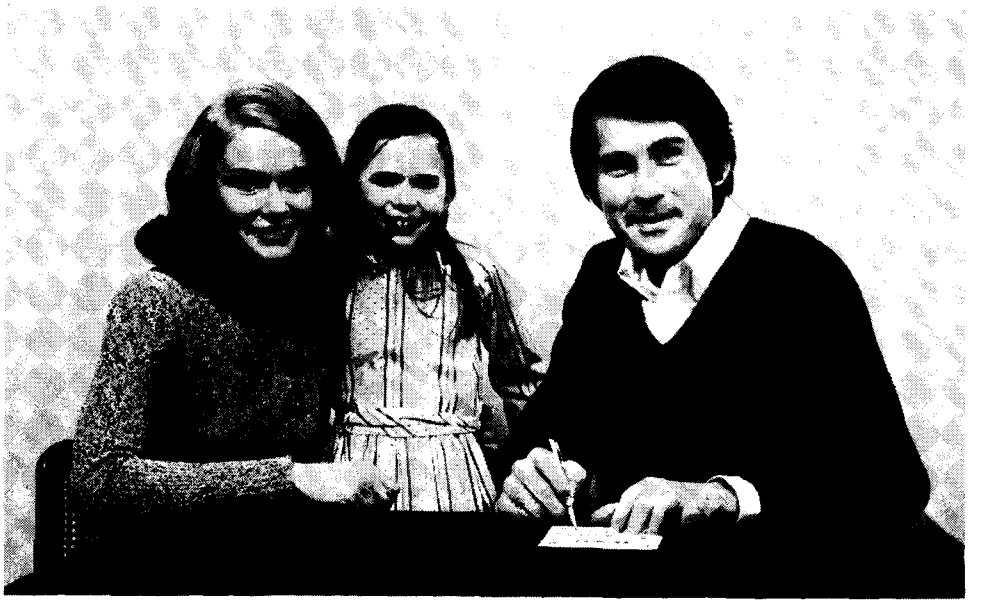


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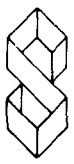
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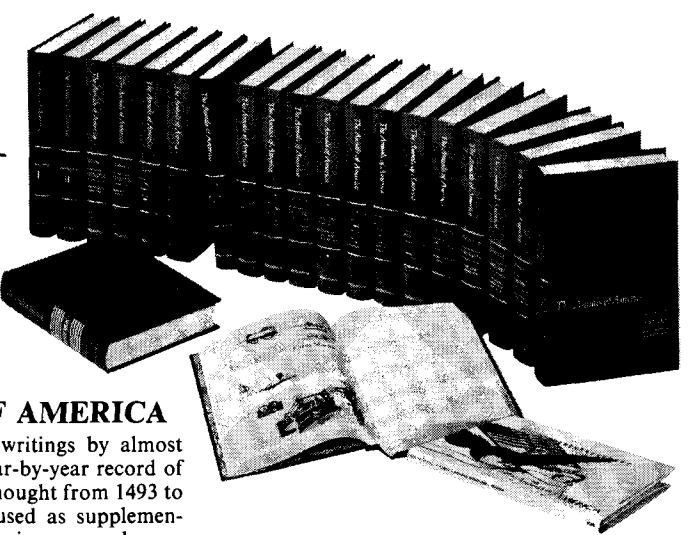
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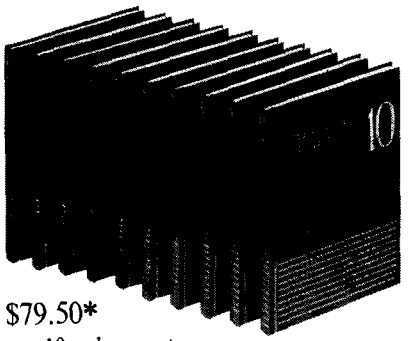
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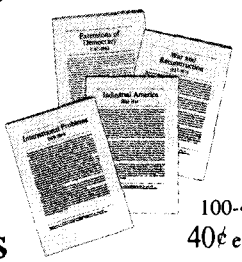


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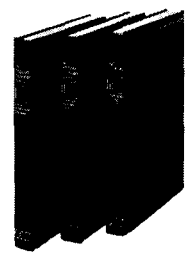


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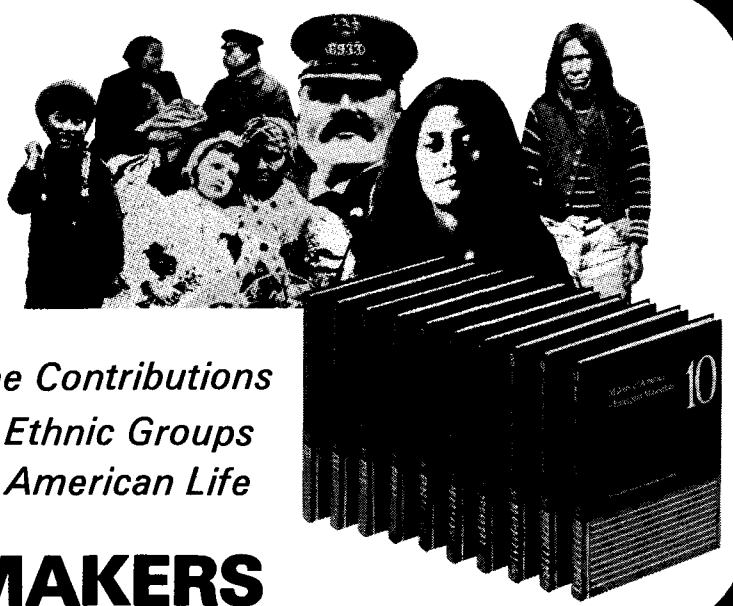
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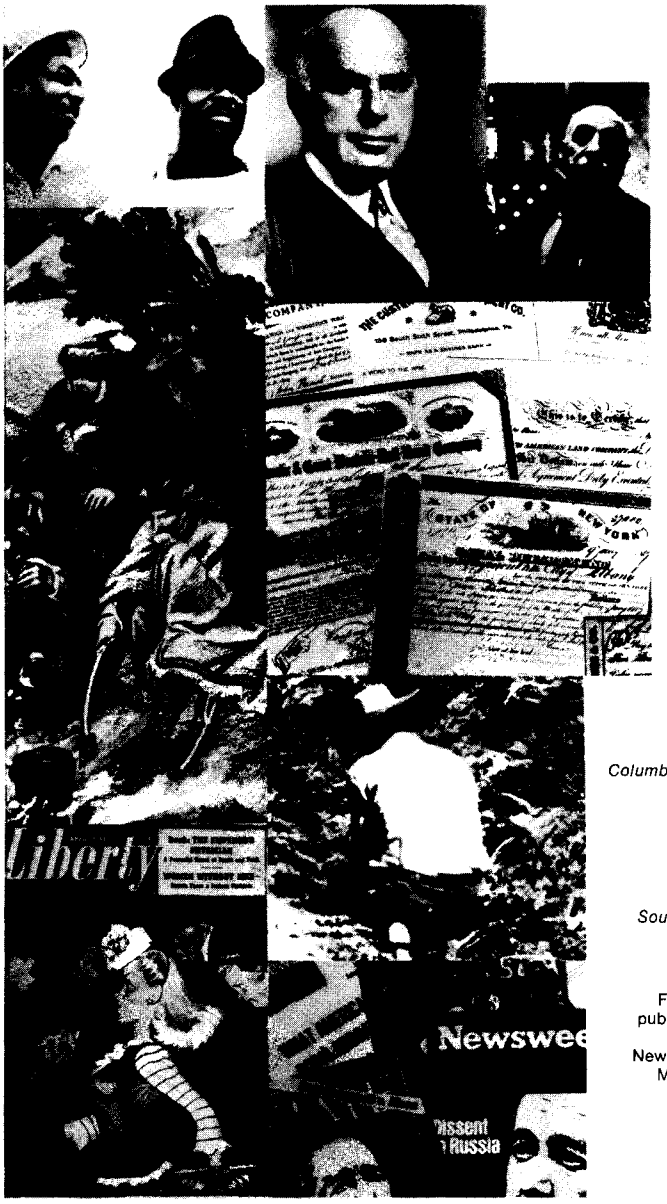
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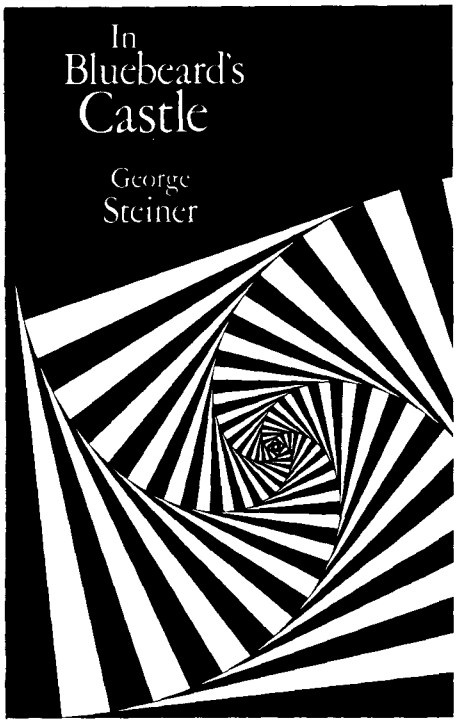
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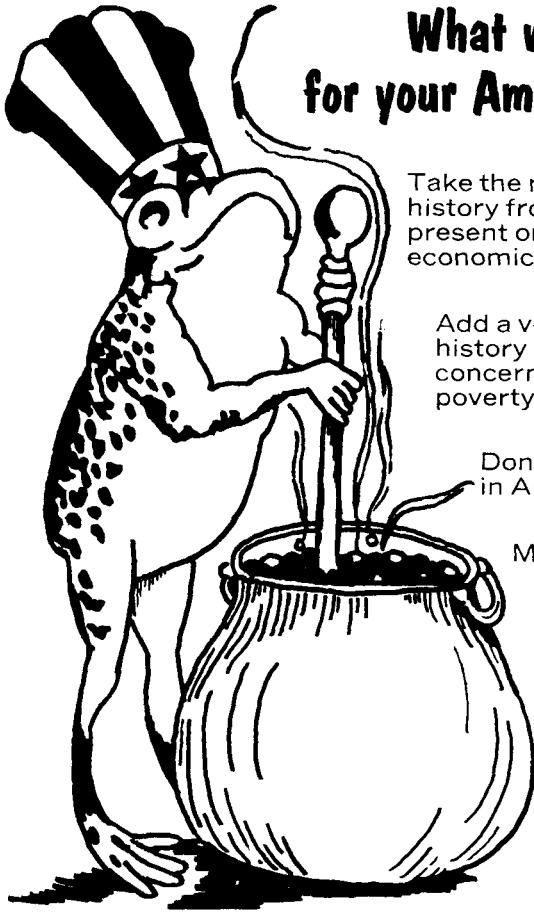
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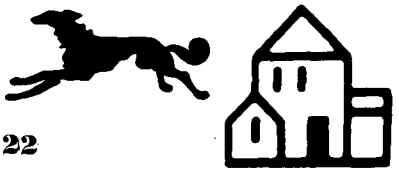
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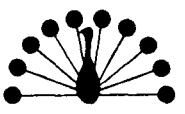
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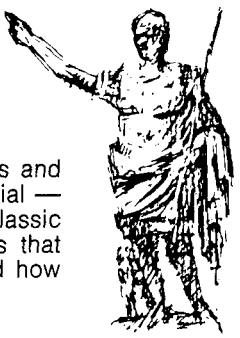
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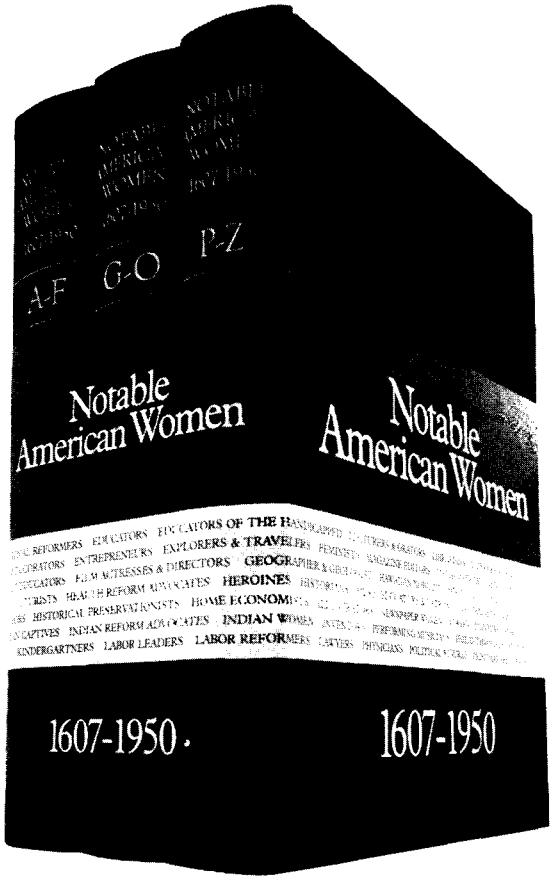
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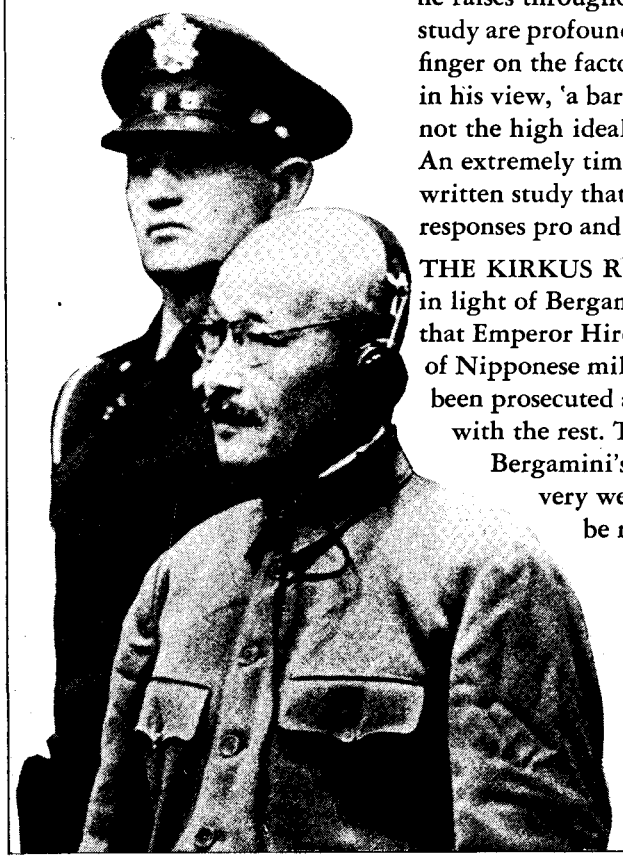
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